Ericsson's Sun Motor, erected at New York, 1883.

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THE SUN MOTOR AND THE SUN'S TEMPERATURE.

By J. ERICSSON.

From "Nature."

THE annexed illustration (Fig. 1) represents a perspective view of a sun motor constructed by the writer, and put in operation last summer. This mechanical device for utilizing the sun's radiant heat is the result of experiments conducted during a series of twenty years; a succession of experimental machines of similar general design, but varying in detail, having been built during that period. The leading feature of the sun motor is that of concentrating the radiant heat by means of a rectangular trough having a curved bottom lined on the inside with polished plates so arranged that they reflect the sun's rays towards a cylindrical heater placed longitudinally above the trough. This heater, it is scarcely necessary to state, contains the acting medium, steam or air, employed to transfer the solar energy to the motor; the transfer being effected by means of cylinders provided with pistons and valves resembling those of motive engines of the ordinary type. Practical engineers, as well as scientists, have demonstrated that solar energy cannot be rendered available for producing motive power in consequence of the feebleness of solar radiation. The great cost of large reflectors and the difficulty of producing accurate curvature on a large scale, besides the great amount of labor called for in preventing the polished surface from becoming tarnished, are objections which have been supposed to render direct solar energy practically useless for producing mechanical power.

The device under consideration overcomes the stated objections by very simple means, as will be seen by the following description: The bottom of the rectangular trough consists of straight wooden staves, supported by iron ribs of parabolic curvature secured to the sides of the trough. On these staves the reflecting plates, consisting of flat window glass silvered on the under side, are fastened. It will be readily understood that the method thus adopted for concentrating the radiant heat does not call for a structure of great accuracy, provided the wooden staves are secured to the iron ribs in such a position that the silvered plates attached to the same reflect the solar rays towards the heater. Fig. 2 represents a transverse section of the latter, part of the bottom of the trough, and sections of the reflecting plates; the direct and reflected solar rays being indicated by vertical and diagonal lines.

Referring to the illustration, it will be seen that the trough, 11 feet long and 16 feet broad, including a parallel opening in the bottom, 12 inches wide, is sustained by a light truss attached to each end; the heater being supported by vertical

plates secured to the truss. The heater is 6¼ inches in diameter, 11 feet long, exposing $130 \times 9.8 = 1274$ superficial inches to the action of the reflected solar rays. The reflecting plates, each 3 inches wide and 26 inches long, intercept a sunbeam of $130 \times 180 = 23{,}400$ square inches section. The trough is supported by a central pivot, round which it revolves. The change of inclination is effected by means of a horizontal axle—concealed by the trough—the entire mass being so accurately balanced that a pull of five pounds applied at the extremity enables a person to change the inclination or cause the whole to revolve. A single revolution of the motive engine develops more power than needed to turn the trough, and regulate its inclination so as to face the sun, during a day's operation.

The motor shown by the illustration is a steam engine, the working cylinder being 6 inches in diameter, with 8 inches stroke. The piston rod, passing through the bottom of the cylinder, operates a force pump of 5 inches diameter. By means of an ordinary cross-head secured to the piston rod below the steam cylinder, and by ordinary connecting rods, motion is imparted to a crank shaft and fly-wheel, applied at the top of the engine frame; the object of this arrangement being that of showing the capability of the engine to work either pumps or mills. It should be noticed that the flexible steam pipe employed to convey the steam to the engine, as well as the steam chamber attached to the upper end of the heater, have been excluded in the illustration. The average speed of the engine during the trials last summer was 120 turns per minute, the absolute pressure on the working piston being 35 lbs. per square inch. The steam was worked expansively in the ratio of 1 to 3, with a nearly perfect vacuum kept up in the condenser inclosed in the pedestal which supports the engine frame.

In view of the foregoing, experts need not be told that the sun motor can be carried out on a sufficient scale to benefit very materially the sun-burnt regions of our planet.

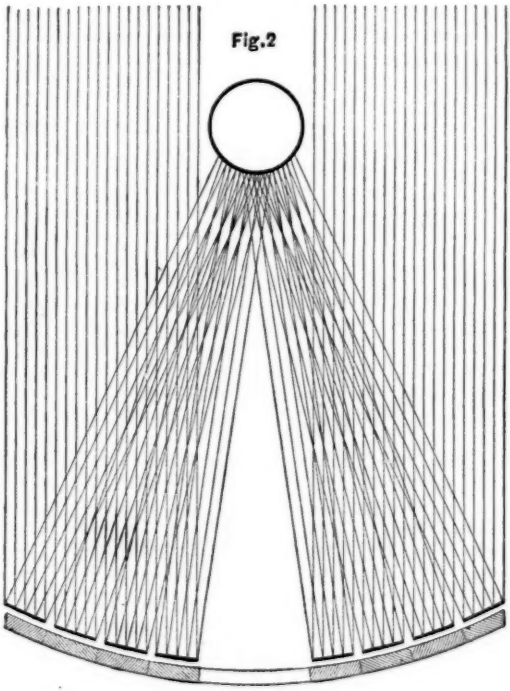
With reference to solar temperature, the power developed by the sun motor establishes relations between diffusion and energy of solar radiation which show that Newton's estimate of solar temperature must be accepted. The following demonstration, based on the foregoing particulars, will be readily comprehended.

The area of a sphere whose radius is equal to the earth's mean distance from the sun being to the area of the latter as $214.5^2 : 1$, while the reflector of the solar motor intercepts a sunbeam of 23,400 square inches section, it follows that the reflector will receive the heat developed by $\frac{23400}{214.5^2} = 0.508$ square inch of the solar surface. Hence, as the heater of the motor contains 1274 square inches, we establish the fact that the reflected solar rays acting on the same are *diffused* in the ratio of $1274 : 0.508 = 2507 : 1$. Practice has now shown that, notwithstanding this extreme diffusion, the radiant energy transmitted to the reflector by the sun is capable of imparting a temperature to the heater of 520° Fahr. above that of the atmosphere. The practical demonstration thus furnished by the sun motor enables us to determine with sufficient exactness the minimum temperature of the solar surface. It also enables us to prove that the calculations made by certain French scientists indicating that solar temperature does not exceed the temperatures produced in the laboratory are wholly erroneous. Had Pouillet known that solar radiation, after suffering a *two thousand five hundred and seven fold* diffusion, retains a radiant energy of 520° Fahr., he would not have asserted that the temperature of the solar surface is 1760° C. Accepting Newton's law that "the temperature is as the density of the rays," the temperature imparted to the heater of the sun motor proves that the temperature of the solar surface cannot be less than $520° \times 2507 = 1{,}303{,}640°$ Fahr. Let us bear in mind that, while attempts have been made to establish a much lower temperature than Newton's estimate, no demonstration whatever has yet been produced tending to *prove* that the said law is unsound. On the contrary, the most careful investigations show that the temperature produced by radiant heat emanating from incandescent spherical bodies diminishes inversely as the *diffusion* of the heat rays. Again, the writer has proved by his vacuum-actinometer, inclosed in a vessel maintained at a constant tempera-

ture during the observations, that for equal zenith distance the intensity of solar radiation at midsummer is 5°48 Fahr. less than during the winter solstice. This diminution of the sun's radiant heat in aphelion, it will be found, corresponds within 0°.40 of the temperature which Newton's law demands. It is proposed to discuss this branch of the subject more fully on a future occasion.

The operation of the sun motor, it will be well to add, furnishes another proof in support of Newton's assumption that the energy increases as the *density* of the rays. The foregoing explanation concerning the reflection of the rays (see Fig 2), shows that no augmentation of temperature takes place during their transmission from the reflector to the heater. Yet we find that an increase of the number of reflecting plates increases proportionably the power of the motor. Considering that the parallelism of the rays absolutely prevents augmentation of temperature during the transmission, it will be asked: What causes the observed increase in mechanical power? Obviously, the energy produced by the increased *density* of the rays acting on the heater. The truth of the Newtonian doctrine, that the energy increases as the density of the rays, has thus been verified by a practical test which cannot be questioned.

It is scarcely necessary to observe that our computation of temperature—1,303,640° Fahr.—does not show maximum solar intensity, the following points, besides atmospheric absorption, not having been considered: (1) The diminution of energy attending the passage of the heat rays through the substance of the reflecting plates; (2) the diminution consequent on the great amount of heat radiated by the blackened surface of the heater; (3) the diminution of temperature in the heater caused by convection.



COMPENSATION FOR CURVE RESISTANCE ON RAILWAYS.

By BEVERLY R. RANDOLPH.

Contributed to Van Nostrand's Engineering Magazine.

In the construction of railways where curves occur upon the heavier grades, the reduction of the rate of grade, in order to compensate for the increased resistance due to curvature is generally considered necessary in order to attain the most economical results in transportation.

The argument being that, if the total resistance which is necessarily opposed to the movement of trains is distributed evenly over the entire line the locomotives will be able to haul larger trains, and, consequently, freight can be carried at less cost. The amount of grade reduction which is necessary or advisable on the various curves to accomplish this result is not established with any certainty. So great is the diversity of opinion on this subject that many engineers still cling to the old practice of no compensation whatever. While this is frequently due to a prejudice against "book engineering," or results from a desire to remain in the old beaten paths, a little consideration of the subject appears to indicate that there are many cases in which it is the true policy.

In the location of every road we find what may be called "controlling points." A stream cannot be crossed at more than a certain height above the water, and the cutting in the next ridge cannot be made greater than a certain depth, and the difference between these two elevations, together with the horizontal distance between them, establishes the character of grade. If, in order to distribute the resistance evenly, the rate of grade is lessened on curves, it must be correspondingly increased on tangents. This even distribution would produce the best results if a locomotive could be made which would make steam as fast as it could use it; but every machine yet contrived, if worked anywhere near its utmost capacity, must ultimately fail for want of steam. In view of this fact it has been suggested that long grades be divided into sections, with short levels in between, on which the locomotive would have an opportunity to make a surplus of steam for use on the next grade section. This would involve an increase in the rate of grade, which the advantage gained for steam making is expected to make up. On a grade line on which no reduction is made, the irregular resistance obtained by the broken grade just mentioned is furnished in a lesser degree. The curves representing the grade sections, and the straight lines the level sections or resting places where steam can accumulate. If on a line with evenly distributed resistance the load must be lessened, on account of the steaming ability of the locomotive, to an amount which does not exceed the load which can be hauled round the maximum curve on the evenly distributed grade, then the advantage of compensation is lost. Provided, of course, that the straight portions of the line afford sufficient relief to make up for the additional steam required on the curves.

I am not aware of any experiments which will throw light on this subject, though the following computations may assist toward a conclusion:

Suppose two grade lines A and B, each two miles long, having a total of one mile each on 8° curve, or 42240° of curvature, the total elevation overcome being 132 feet. Suppose line A to be compensated on a basis of .03 per degree per one hundred feet, then

Resistance on A = 1.37 per cent. regular grade.
Resistance on B (straight) = 1.25 per cent. regular grade.
Resistance on B (curve) = 1.49 per cent. regular grade.

With a load of 400 gross tons we will have

		lbs.
Resistance A	Due to grade	10,960
	Due to friction, wind, etc.	1,400
		12,360
Resistance B	(straight) Due to grade	10,000
	Due to friction, etc.	1,400
		11,400
Resistance B	(curve) Due to grade and curve	11,920
	Due to friction, etc.	1,400
		13,320

Ratio of driving rod leverage to wheel diameter..... 4.10

Total piston area, 309 square inches.

		lbs. per in.
Pressure on piston.	A 30,900 lbs.....=	100
	B (straight) 28,500 lbs.....=	92.2
	B (curve) 33,300 lbs.....=	107.7

Let n represent the number of cubic feet of steam used in making ascent on line A, then $\frac{1}{2}n$ will represent that used on B (straight) and B (curve) respectively. If, now, we suppose engines to be worked at the proper point of cut-off, so that the steam is admitted to the cylinder at the pressure indicated above in each case, a thing which must be accomplished either with the cut-off or throttle, in order to maintain an even speed, we will have the following number of units of heat absorbed in the work:

A.....	$17.529n$
B (straight)....17271.8 $\times\frac{1}{2}n$	$=8635.9n$
B (curve).....17697 $\times\frac{1}{2}n$	$=8848.5n$
	$17,484.4n$
Excess required by compensated line	$44.6n$

The number of heat units here given are from "Nystrom's Mechanics," 1880.

The uncompensated line would therefore require a lower duty from the boiler, and the same locomotive could haul a larger load than on the line where the resistance is evenly distributed.

Further investigation proves this difference to vary approximately as the square of the amount of curvature.

It may be observed that, following this line of reasoning, the greater the variations in the resistance the greater would be the capacity. Under certain circumstances this is true, as mine managers find it is much more economical to haul their output to the foot of a shaft and then hoist it, than to bring it out by a slope, though part of this is, of course, chargeable to more extensive repairs and additional friction. This fact would indicate that the method first suggested with grade and level sections would be productive of good results. On the other hand, the difference in the amount of heat required is relatively so small that it would be inappreciable in practice. Theoretically, however, it is clearly on the side of no compensation. Where the steaming ability of the locomotive controls the load, compensation for curvature would therefore be worse than useless. To what extent this is the case can only be roughly determined in practice, though discussion of it may convey some idea.

We will take, for example, the consolidation locomotive, exhibited by the Bald-Locomotive Works at the Chicago Exposition. This machine is described in the *Railroad Gazette*, June 22d, 1883, the characteristics with which we are concerned being,

Total weight .....lbs.	114,000
" weight on drivers.....lbs.	100,000
Cylinders.....inches	20×24
Diameter of driving wheel.....inches	49
Grate surface.....sq. feet	30

From which it will be seen that the tractive force is sufficient in average weather for the assumed load.

Contents of each cylinder..cubic feet	3.63
Steam per revolution.....cubic feet	15.52
No. of revolutions in two miles.... $\frac{10560}{12.83}$	$=823$
Amount of steam used...15.52×823=	10949.96
Cubic feet of water evaporated $\frac{10949.96}{290}$	$=413$

Supposing the boiler to evaporate 15 cubic feet of water per square foot of grate surface per hour, we have $30\times15=450$, or 112.5 cubic feet in 15 minutes consumed in ascending the supposed grade at eight miles per hour. There would be, therefore, a heavy draft on the amount of heat previously stored in the boiler. By reducing the speed to two miles per hour the steam could be furnished, if the boiler could still produce at the same rate, but as we would then considerably reduce the draft, we could not expect the same rate of production.

Among practical engine runners there seems to be a prevailing impression that a locomotive fails for want of steam in about two miles, or, in other words, that the surplus energy which can be accumulated on a level at the foot of a grade will supply what the boiler fails to provide for this distance. Some experiments made on the East Tennessee, Virginia and Georgia Railroad, February 15th, 1882, and reported in the *Railroad Gazette*, March 31st, 1882, will show a much lower limit.

In this trial four different locomotives of nearly the same class were tried over the same ground, with the same trains being run up the grade until stalled.

The following extracts from the tabulated report will give a fair idea of the results attained:

No. of engine.	Maker.	Type.	Cylinder.	Weight on drivers in lbs.	Total Weight.	Gross load.	Distance made.	Curvature where stalled.	Time—minutes.
92	Baldwin.	Ten-wheel.	18×24	60,000	82,750	1,003,900	3820	6°	7
82	Rogers.	"		62,100	86,600	same.	3600	6°	6
34	Baldwin.	Mogul.		66,000	78,000	same.	3394	Tang.	5
79	Rogers.	Ten-wheel.		62,100	86,600	same.	3376	Tang.	6

Both grade and curvature are reported, the former being very irregular, and ranging from level to 79.7 feet per mile on the portion included above, while the latter ranges from 3° to 10°. Unfortunately for the purpose of this paper the situation of the curves is not reported, though from some remarks made in the editorial of the same number it may be assumed that in the trials here given, the 10° curve was not reached, though it was passed in some further trials with lighter loads, it being 100 feet long and between 4,200 and 4,300 feet from the foot of the grade.

Two additional sets of trials were made, one with 903,100 lbs. gross load, with which neither locomotive reached the top of the grade, though all but No. 82 passed the 10° curve on a 98 feet grade; the other with 851,700 lbs. gross load, in which all four locomotives reached the top, 5,700 feet.

Here it is very evident that some factor, other than the tractive power, controlled the load, since the locomotive with least weight on drivers, passed the points where the heavier ones stalled, and three locomotives passed the point of greatest resistance, but were unable to reach the top. Since the object of reducing the grade on curves is to increase the tractive capacity of locomotives, it would here, in a grade 5,700 feet long, have had no effect in increasing the load which could be carried over the grade, and the arrangement which would require least steam would undoubtedly be best.

Leaving this subject we will now pass to the matter of compensation without regard to its advisability.

The numerous discussions of this subject which have appeared in the various technical journals and books in the last few years, seem to have done comparatively little to clear up the difficulty, and the inquiring engineer finds a different opinion in each authority which he examines. From one he gets a constant which is to be multiplied by the degree of the curve, another tells him this constant should increase with the degree of the curve, while a third gives him a long formula, and when he comes to make the substitutions for the unknown quantities he finds as great a variety of values in the practical data as he has previously found opinions in authors.

A little consideration of the various factors which must enter into any formula, whether theoretical or empiric, will, I think, disclose good reasons for this disagreement.

The common truck or bogie with which we have to deal may be described as a four-wheeled wagon, all wheels rigid on the axles, the propelling power applied at the central point of the wagon, the guiding being done by the rails. The force required to drag this wagon through a curve will vary,

First. With the length of the truck, since the wheels are rigidly attached to the axles, and consequently to one another the effort to preserve the straight line in their motion causes the flange of the forward outside wheel and the after inside wheel to impinge on the rail. For experiment on this subject see *Van Nostrand's Magazine*, Vol. XXIV., page 390.

This flange friction is a fruitful source of resistance, and is decreased by the draught, acting with the flange contact of the after inside wheel as a fulcrum to draw the forward flange away from the rail. Its effect in so doing will depend on the angle which the line of draught makes with a line drawn from the pin to the point of contact of the flange of the after inside wheel, and on the distance from this same point of contact to the front wheels, the former controlling the lever arm of the power, and the latter of the resistance or weight to be moved, both being dependent on the length of the truck, though not bearing the same ratio to it.

Second. With the tightness or looseness of the gauge. The additional width or "play" allowed in the gauge on some roads, by permitting the truck to assume a greater or less angle with line of draught, varies the resistance. For practical demonstration see *Van Nostrand's Magazine*, Vol. XXIV., page 517.

Third. With the length of the train. The draft acting on each truck of a train is the resultant of the traction exerted through the truck ahead of it and the resistance from the truck behind. Since there is approximately a regular increment added at each truck, the ratio of these two forces will continually decrease toward the head of the train, and the resultant will therefore continually approach a direction radial to the curve, thus continually varying the angle of draught and the resistance, as before explained. Up to a certain point this will diminish the resistance by drawing the forward wheels from the outer rail and lessening the flange friction. After this point is reached the forward wheels will come in inner contact, and the flange friction will increase from this point forward.

Fourth. With the velocity of the train. Owing to the unequal length of the two rails on a curve, and the fact that the wheels are rigidly fastened, one wheel must slip, and we have resistance in the force required to produce this slipping. With sufficient velocity the centrifugal force throws so much of the weight on the outside wheel that the inner wheel slips more or less readily, in addition to which the flange friction is also effected by the extent of the centrifugal force.

Fifth. With the super elevation of the outer rail. By means of this super-elevation the force of gravity is brought in to counteract the centrifugal force, and also to effect the direction of the draught through its horizontal component, which would cause the truck to slide down the inclined plane toward the center of the curve. This super-elevation varies constantly with the opinions of the parties who have charge of the maintenance, and with the attention it receives from the section men.

Here we have five factors all effecting the result of any experiments made to determine curve resistance, and all varying for different roads and parts of roads, through frequently very wide limits. Hence it would seem that anything in the shape of an exact universal formula is scarcely to be expected in the present state of railroad construction. But railroads must be built, and the practical engineer must find some way out of the difficulty, however rough it may be.

In the matter of the length of truck we may hope for some uniformity, as the influence of the Master Car Buiders' Association grows and their standards are adopted, provided they do not change their standard as the years pass by, and leave us with a mixture of old and new standard trucks.

For uniformity of gauge we have not so hopeful an outlook. Opinions will probably differ on this subject so long as railroads are used, and even should the necessity for widening be universally admitted the amount will still be a question of dispute, and as the man who builds is not often the man who maintains, the construction will be without any definite guide as to what is best for him to do.

In determining the length of trains we have two important factors to consider, one, the constantly increasing size and tractive power of the locomotives tending to long trains, and the other, in the constantly increasing load per car without a corresponding increase in the length of the car, which results in shorter trains. Here, again, we are at sea, and the best thing to be done in view of future contingencies must depend a good deal on bold guessing.

In the matter of velocity, the coast is more clear, since, if the other elements are fixed, an intelligent operator would not be long in finding the best speed at which to run his trains, which, supposing the entire calculation to have been correct, would be the speed originally intended. There is, however, an element of uncertainty in the future for this matter. As before explained, the principal cause of resistance which is effected by the velocity is the slipping of one wheel, owing to the rigidity of the wheels on the axle. Experiments show that after a speed of twenty-five miles per hour has been attained the effect of this slipping disappears. Unfortunately I have not the data which would enable me to state whether they were made with long or short trains, or both.

If with short trains they are very readily explained, on the supposition that the centrifugal force takes all weight off the inner wheel at that speed, thereby allowing it to slip without resistance. With long trains, on the other hand, we would expect the flange friction from the inner contact in the forward part of the train, produced by the draft as before explained, to cause the inner wheel to slip with difficulty. However this may be, the speed at which the majority of cars pass over a railroad, especially up heavy grades, is near eight miles per hour, and we must expect a considerable increase of power to be required in order to produce the necessary slipping of the wheel. This difficulty a good "loose wheel" would of course, obviate, and would also do away with a great deal, if not all of the flange friction from the outer contact of the leading wheels. Just why something of the kind has not come into use, I have never been able to find any one who could explain. The objection seems to be largely a practical one, simplicity of construction and cheapness of maintenance being the main points. There have been, however, several patents secured for wheels which seem exceedingly simple, and though the repairs would, of course, exceed those of the common wheels and axles, yet, on the other hand, the advantage gained for freight service especially would be considerable. If a good contrivance of this nature should be invented, or one of the present ones come into use, roads on which the grades were reduced on curves to satisfy the present condition would reap no benefit from the new state of affairs, since the grades on which their straight portions are built establishes their maximum resistance. It may be argued that as nothing of the kind has yet appeared, nothing is likely to, but the same argument has been used against the steam-boat, railroad, telegraph, and innumerable other contrivances which are every day demonstrating their practicability. The thing may even now be in existence, and only awaiting the energy of some railroad officer sufficiently sanguine and strong to give it a fair trial. The present furor for cantilever bridges gives an idea of how a thing of the kind is taken up when once made a success.

When we come to compare the duty of locomotives drawing fixed and loose wheels, with reference to their steaming capacity, the advantage of the loose wheel seems to disappear, although very apparent in detail experiment. Tests with the loose wheels of the Miltimore Car Axle Company, made on the Central Vermont Railroad, in 1874, by Mr. Henry Waterman, show a saving of 23 to 48 per cent. in the power required to pass a 3° curve on a grade of 32 feet per mile, and a 4° curve on a grade of 41 feet per mile, the percentage of resistance being calculated on the total resistance while passing the curve, after deducting the influence of grade, without deducting that which is due to journal friction, wind, etc., and is found on straight as well as curved line.

Making allowance for the fact that the experiments were made for the Car Axle Company, and that the experimenter naturally desired to make as good a showing as possible, we will call the saving 30 per cent.

Applying this to line "B," mentioned in the earlier part of this paper, we have,

B (straight)	Due to grade.....	10,000
	Due to friction, wind, etc	1,400
		11,400
B (curve)	Due to grade and curve..	11,344
	Due to friction, wind, etc.	1,400
		12,744 lbs.
Pressure on pistons	(straight).....	28,500
	(curve).....	31,860
Area of pistons, 309.		
Pressure per sq. in.	(straight).....	92.2
	(curve).....	103.1
Units of heat per cubic feet of steam (straight).....		17271.8
Units of heat per cu. ft. of steam (curve)		17627.5
		2)334899.3
Average.....		17449.7
Average with rigid wheels, as previously calculated.....		17484.8
Saving per cubic feet of steam.		35.1

This is about .002 of the total number of heat units required, and would therefore allow an increase of .8 of a ton to the assumed load of four hundred tons. This is, of course, a mere nothing, and will account for the lukewarmness of the railroads in adopting the Miltimore Axle, since it has been quite thoroughly tested, and, so far as I know, is still in use without having been generally adopted.

In the matter of super-elevation, the

constructing engineer is, of course, completely in the dark as to what will be done by the operating department, so his estimate of this factor is a mere guess.

From the foregoing, it would seem that the chances of obtaining an exact expression of the additional resistance caused by curvature is exceedingly remote, if not altogether in infinity. Even if a thoroughly correct formula could be constructed, the uncertainty of the data to be obtained would render its practical application more or less misleading, and the end would probably be as well obtained by a rough general average of all opinions as in any other way.

After reviewing the whole field the question still remains—What is best to be done by the working engineer who must build at once? The case seems scarcely to be made out either for or against compensation for curvature. The arguments in favor of compensation appear to have confined themselves to the supposition that a locomotive can exert its full tractive force throughout the gradient. and have thus constructed general results out of detail experiments. The other side has also proceeded in much the same way, having accepted laboratory experiments as to the number of units of heat required to produce certain effects. Both need practical verification, which will no doubt be furnished in the future from some of the numerous roads recently built with compensated curves. So far, the two sides seem about evenly balanced, though there are still arguments on the side of no compensation.

The constructing engineer should bear in mind that he is not building for the present alone, but for all time. He should therefore consider the probable future of his work in connection with the actual present. If we take a common four-wheel road wagon on a smooth, hard road, we find that the friction on the shortest turns is not appreciably greater than on a straight line. With a railroad truck we have the smooth, hard road, but owing to the rigid wheels and the method of guiding, we have a very much increased friction. It would therefore seem but natural to expect that the future will bring a contrivance which shall pass a curve with as little frictional resistance as road wagon does now. There is certainly no little incongruity in building a practically everlasting structure, like the permanent way of a railroad to satisfy the evanescent conditions of its rolling stock, which lasts at most, but a few years, and is almost always replaced by a different type. Let the rolling stock conform to the permanent way.

A more present and tangible advantage of no compensation is the fact that for descending trains the acceleration is more regular, and the trains can therefore be handled with more safety and less wear of rolling stock.

All things considered, it would seem that an exceedingly conservative policy is the safest, and that if compensation is attempted at all, it should be to as small an extent as is consistent with any results of reliable experiments. If, however, it should be necessary to locate a sharp curve very near the top of a grade it would be good policy to reduce the grade, if it could be done without much increased expense, since the boiler pressure would here be near the lowest limit at which it could move the train, and a very slight additional resistance might determine whether a train could pass over the summit or not.

Another point of interest in this connection, though scarcely germain to the subject of the paper, is, that since the steam pressure is high at the foot of the grade and low at the top, considerable economy can be effected by using the steam expansively from a high boiler pressure at the foot of the grade, hence sharp curves and consequent high resistance is not so objectionable on the lower as on the upper portions of the grade.

ASBESTOS ENAMEL.—Powdered asbestos is used by M. Erichsen, of Copenhagen, for making an enamel or coating to be applied to pipes, walls, and so on. The powder is mixed with soluble salts, such as silicate of potash, and mineral or other colors which combine with silicic acid, so as to form a product which resists the action of oxygen, heat, cold, or damp. The coating furnishes a refractory glaze, which protects the material it is applied to, whether wood, gas or water-pipes, and stone or brick buildings. When applied to masonry or wood the surface of these is first washed with soap and water. In preparing the enamel the refuse asbestos only need be employed. It is also proposed to apply the coating to boilers in order to protect the plates against a too intense fire.

ON THE COMPARATIVE ENDURANCE OF IRON AND MILD STEEL WHEN EXPOSED TO CORROSIVE INFLUENCES.

By DAVID PHILLIPS, M. Inst. C. E.

From Proceedings of the Institution of Civil Engineers.

II.

DISCUSSION.

Dr. C. W. Siemens said, it perhaps would have been better if the discussion had been commenced by persons more interested in the use of iron and steel, than by those who, like himself, were intimately connected with their production; but in another respect it might possibly save the time of the Institution, if he took that early opportunity of referring to some conclusions in the paper with which he could not agree. The author had given the results of elaborate experiments on a subject which was of the utmost importance to engineers; and if his conclusions were to be relied upon, engineers were daily committing a grave error in using a material which gave so slight a guarantee of endurance. But while he accepted every one of the experimental facts adduced by the author, he thought he was in a position to prove, from the author's figures alone, that his conclusions were entirely erroneous. He had referred, in the first place, to a long series of experiments made by the Admiralty, under the direction of Admiral Aynsley, and as far as the collection of facts was concerned, nothing could be more conscientious or thorough than that series of experiments; but as regards proof, they went no further than to show what every experimenter ought to avoid, and how he ought not to conduct his experiments in the future. The author had placed before the Institution the apparatus then used. It consisted of thirty-eight tubes of iron and steel riveted in metallic contact with the shell of a boiler, and exposed partly to air and partly to hot water and solutions. Although the author had a very poor opinion of electricity and its effects, Dr. Siemens had a strong belief in electricity wherever it had a chance of acting for good or for evil, and he was convinced that the results obtained in those experiments were rendered entirely unreliable through galvanic agency. The results were most variable. Whereas one iron (common iron seemed to be the best) gave a corrosion of only 7 grains, on an average per square foot, Bessemer steel gave 21 grains, or three times the amount during the time of exposure. The author was really most merciful when he stated that the result was only 69.3 per cent. in favor of iron, because it was really 300 per cent. in that instance. Notwithstanding these experiments, the Admiralty had adopted a mode of action which seemed strangely at variance with the conclusions to which the experiments would point. They now used steel almost to the exclusion of iron, and he hoped that some one connected with the Admiralty would state the result of more recent experiments undertaken with a better knowledge of the conditions under which they should be made. He believed the conclusions since arrived at were very different from those deduced by Admiral Aynsley some years ago. At Table VII. the author had compared Landore metal and iron, the one giving an average loss per square foot of 506.24 grains, and the other of 483.17 grains, showing a difference of only 4.8 per cent. against the steel, and that was, at any rate, a great deal better than 69 per cent. On the same page the author stated: "The only peculiarity worth noticing in this experiment is that, while the two plates in the feed-water heater lost 381.8 and 394.2 grains, the two in the boiler fed from the heater lost only 8.0 and 3.4 grains respectively." Therefore in the boiler the iron lost by corrosion about one forty-eighth, and the steel about one hundred and twentieth of what they respectively lost in the feed-water heater, the loss of iron in this case being about two and a half times that of the steel. In Table IX., set 98, the Y steel produced by the Siemens process gave a corrosion of 1,220 grains, and the DD. Yorkshire iron 1,221 grains, in each case per square foot of surface; the corrosion in those

instances being practically the same. In the next set, 99, the Y steel gave a corrosion of 259 grains; Staffordshire iron 269 grains, and DD. Yorkshire iron 260 grains, showing that the steel came out best in that series. In set 102, the Y steel gave 109.5 grains (in rain water), and the DD. Yorkshire iron 144 grains. And yet the author followed up these facts with the conclusion that steel corroded on an average 64.8 per cent. more rapidly than iron. He entirely objected to the mode of reasoning adopted; he contended that averages were only applicable to errors of observation. If an observer was not certain of his weighings, and he made a hundred weighings of the same piece of iron, he would be pefectly justified in taking the average. But nothing could be more unscientific or erroneous than averaging several materials, one group of which he chose to call iron, and another which he called steel. It was as if a moral philosopher wanted to find out whether fair complexioned people were more virtuous than dark complexioned people, and were to take six fair people and six dark people promiscuously; then finding that they were all very well behaved, except one of the fair people, who happened to have just escaped from gaol, and had committed six murders, he were to draw his average and say: "I find that fair people have committed, on an average, one murder each, and should therefore not be trusted." That was the kind of argument which the author appeared to have adopted. There were substances, compounds of iron, manganese and silicon sold for steel, which no doubt corroded very rapidly; but it was for the consumer not only to select the proper material, but also to see that it was properly used. He believed it was in regard to the proper selection and use of the materials that the enormous discrepancies with which they had to deal would be found. The author stated that there was more cinder in iron than in steel, and therefore that there was *prima facie* ground for supposing that iron would corrode more than steel. There was, however, an essential difference between cinder in iron and the scale of steel. The cinder in iron was a glassy substance, which was a dielectric, and therefore had no effect upon the corrosion of the metal, whereas the scale on steel, which was produced in rolling, had a very deteriorating influence; it was a magnetic oxide, which was negative to the steel, and wherever the metal was exposed in the presence of such magnetic oxide, corrosion took place rapidly. Again, if the scale should be rolled into steel plates, as was sometimes the case, rapid corrosion ensued, for the same reason. But he need hardly say that with proper care those causes of undue corrosion could be and were prevented, and the extensive use to which steel was now put proved sufficiently that there was, at any rate in ordinary practice, no such destructive effect going on. The author stated that those interested in steel had been singulary negligent in not following up the question of corrosion. Being himself much interested in steel, Dr. Siemens had for some years caused a running set of experiments to be carried out by Mr. Willis, the chemist at Landore, which told a very different story from the author's. In one series, extending over six months, made partly in a boiler supplied with salt water, and partly by exposure in a tidal river—the plates being exposed to the air for six hours, and then immersed for six hours in salt water—the result was in some instances of open exposure slightly in favor of iron, but, in the cases of boilers, always very much in favor of steel. He had just received a report from Mr. Willis, in which reference was made to a point of importance, the perfect cleaning of the surfaces. It had been found that if the surfaces were carefully cleaned of oxide by dipping the plates in the first instance in an acid solution, the corrosion was always much diminished. The evil effects of scale on steel were pointed out at the Institution of Naval Architects, by Mr. Barnaby, on the 5th of April, 1879, when he clearly showed that the magnetic oxide scale was very deleterious in its effects. He believed that it was now the practice of the Admiralty to clean the scale off before using the plates for shipbuilding. During the past week he had received a number of letters, quite unsolicited, from gentlemen interested in the use of steel, all speaking in the most definite manner in favor of steel as a metal not liable to corrode under ordinary circumstances. One of them was from the Clyde Bank

Foundry, in which it was stated by Mr. Thompson that forty steel boilers had been at work for more than two years, and that their examinations had led to the conclusion that no active corrosion was going on. He believed it would be found, from general experience, that steel under proper conditions lasted at least as well as iron. He hoped that the discussion would bring out such further facts as would put the question practically at rest. That there was, under certain conditions, a very active corrosion going on both upon steel and iron was clearly proved by the paper, and by other experiments; but the conclusions drawn by the author in favor of iron were, he thought, unjustified by the results of his own experiments as well as of others.

Mr. B. Martell thought engineers were much indebted to the author for bringing his valuable experiments before them. When it was remembered that at the present time 82,000 tons of steel ships were being built, the great importance of the subject would be at once recognized. The question of the mechanical properties of mild steel was no doubt well understood. It was known that it could be produced possessing all the qualities required for ship purposes (he referred especially to the hulls of ships), with all the ductility and strength, as compared with iron, that could be hoped for in a material of that kind. The question of corrosion, however, was one that required to be solved, and for that purpose more information was needed than had been hitherto obtained. Many shipowners were anxious to build ships of steel, but an opinion was abroad that it deteriorated more quickly than iron; hence the importance of having reliable facts upon which to form a correct opinion on that branch of the subject. About eighteen months ago a steel ship in the North of England, not a year old, was hauled up on a slip-way; he went there for the purpose of examining her, and his examination appeared to bear out in a striking manner one of the results mentioned by the author, who had shown the rapid deterioration that took place where steel was exposed alternately to salt water and to air. The vessel was riveted with iron rivets, and he found that between the light water mark and the load water mark, which was alternately wet with sea-water and then dry and exposed to the air, a rapid deterioration had taken place as compared with the other parts of the vessel, and with iron vessels; in fact, the steel round the rivets had wasted to a considerable extent, so that the rivet points were protruding some distance beyond the steel. He thought it might probably be due to galvanic action. Dr. Siemens had referred to magnetic oxide, but that could not have been the explanation, because by hammering the rivets the whole of that would have been certainly beaten off. He attributed it more to the galvanic action taking place to some extent between the iron rivets and the steel plates. The result seemed to show that under such conditions as those to which he had referred a more rapid deterioration ensued than under other circumstances. Two months ago, however, that same vessel, which had been continuously running since, was hauled up again and thoroughly examined, and owing to the greater care taken in protecting that part, no deterioration of any moment had taken place more than in any other part of the vessel. A striking and important fact was at the same time brought out. The vessel was constructed with a double bottom, or water ballast tank, extending fore and aft, and inside that no deterioration had taken place beyond what would have occurred in iron, and possibly less. That seemed to show that where steel was entirely immersed in water no more rapid deterioration followed than in iron. He was of opinion that the results stated by the author might be correct, but they would not prevent his adopting steel for ships in preference to iron. Mild steel was superior to iron for every purpose for which it was used mechanically, and by continually coating the surfaces it could be protected as much as iron. With regard to chemical action, he had known an iron vessel carrying sugar, in which some of the bottom plates had been eaten through in a few months, and nothing could be worse than that with the use of steel. But there, again, there existed an excellent preservative in Portland cement, which would protect the bottom from any action of that kind. The inside was open to observation, and it could always be coated. By the use of steel the ship was stronger, and most

of the parts could be protected from deterioration, with the exception of that between the light and the load line, and that only required a little closer attention in keeping it coated so as to prevent rapid corrosion. He hoped the result of the paper and of the discussion would not be to scare shipowners from the use of steel. He trusted it would not be considered, as the result of experience, that steel had deteriorated 120 per cent. more than iron, as would appear to be the case from some of these experiments; but that the facts derived from the actual wear and tear of ships would be considered before any decisive conclusion was arrived at.

Mr. N. Barnaby, C. B., drew attention to the statement in the paper: "In spite of these reasonings, it is undoubtedly a fact, that under almost all circumstances iron, and particularly the harder classes, is far superior to the finer steels in its resistance to corrosion, and this the experiments described by the author incontestably prove." When he saw that paragraph he remarked to the engineer-in-chief of the Admiralty, Mr. Wright, who was a member of the first Boiler Committee (of which the author was also a member), and had continued the experiments to the present time, "it is necessary that there should be some explanation with regard to this, because we are using steel shells for boilers very largely, and people will expect that some one from the Admiralty should say whether this statement, derived from Admiralty experiments, has been borne out by later work." Mr. Wright replied: "We have continued the experiments from the time when Mr. Phillips left the Boiler Committee, and we have come to the conclusion that there is no difference in the rate of corrosion between iron and the mild steels we are using. By such experiments as those which he conducted, and which have been continued since, so far as we can make out, the results are pretty nearly the same." He stated, moreover, that steels could be as well protected as iron by zinc suspended in boilers. That was the justification for the present practice of the Admiralty in using mild steel for the shells of boilers. That practice was a very recent one, the Admiralty having commenced making steel boilers only within the last few years; and it was hardly right that he should speak of it when there were so many others who had had a long experience of the use of steel in boilers. His only justification in alluding to the subject was that engineers might wonder how it happened that the Admiralty report, as the author had presented it, should not be in accordance with the present Admiralty practice. With regard to what Dr. Siemens had said as to the effect of the hard black oxide upon the surface of steel, it was true they discovered a long time ago that the effect of that oxide was very strong indeed, almost like that of copper, yet they were foolish enough (he could use no other word) in building two ships at Pembroke, to allow them to be coated with anti-fouling composition before the black oxide was completely removed. His excuse was that the portions of black oxide remaining were very small, and that the officers who were charged with the duty of getting the bottoms quite clean thought they had done so. It was not until the Iris had been at sea for some months that it was discovered that rust was forming under the coat of paint with which the bottom had been covered. The Admiralty had learned wisdom by that occurrence, and he believed it would not happen any more. They now took pains to clear the oxide off completely. He thought the reason why they had found it out before others had discovered it was, that it was the practice in private trade to build vessels of that kind in the open air, and it was there easier for the black oxide to get removed from the surface of the steel. The Admiralty built their ships under cover, where it did not come off so easily.

Mr. J. Farquharson said he had a memorandum showing the importance of removing the oxide. The Admiralty were aware of the view set forth in the paper, that mild steel was much more liable to corrosion in salt water than iron, long before the Report of the Boiler Committee was printed, and they determined to test that point specifically. In looking at the diagrams he was of opinion that no reliable inference could be drawn from such a combination; it was so complicated and was affected by so many conditions. By Mr. Barnaby's directions it was arranged that they should try—not

surfaces partly covered with oxide and partly uncovered, in an apparatus the condition of which they knew nothing about, and which was itself a different metal—but plates of iron and steel of a considerable size carefully prepared, the oxide being entirely removed by two processes, first pickling it off by chemical means, and secondly, planing it off to see how far the process of removing it affected the result. The plates had been tried under conditions which had led to the inference that iron corroded a great deal more rapidly than steel; but the result of the experiment when the plates had been divested of oxide on the surface, was that they were practically alike; if there was any difference it pointed in favor of iron. It was a rather extended series of experiments carried on at Portsmouth with great care. It was then determined to ascertain something about the electric effect of the oxide, and another series of plates was prepared, each 2 feet long, 1 foot wide, and $\frac{3}{16}$ inch thick of mild steel. Some of the plates were prepared in the way he had described—the oxide being removed by two processes. The plate from which the oxide had been pickled lost 4 oz. and 70 grains; the one from which it had been removed by planing lost 3 oz. and 390 grains. That was the case with two pieces of the same plate. In another experiment, one plate with the oxide removed as he had described was combined with another plate of the same size, 2 feet long and 1 foot wide, which had been cut from the same piece of Landore steel; they were placed parallel to each other, 3 inches apart, in a wooden frame, and were connected by wire, one with the oxide on, the parts where the oxide was not on being touched over with a protective varnish to prevent local action. The plate with the oxide on lost nothing; the plate in contact with it, combined electrically, lost 10 oz. 95 grains. He had before him the record of another series treated in the same way, but he would only refer to a few instances to show that there was a great amount of harmony in the results. A plate with the oxide pickled in contact with another with the oxide on, held parallel to each other 3 inches apart, immersed in the same way in Portsmouth Harbor, lost 12 oz. and 34 grains; that with the oxide on lost 75 grains—that happening from minute parts unobserved and unprotected by the varnish pitting. With another pair of plates, one with the oxide removed, combined with a copper plate of the same size, and placed in precisely the same relation to it as had been the plate with the oxide on, lost 11 oz. 345 grains—less than one of the other plates where steel alone was used, showing that a tolerably compact coating of oxide was as detrimental to steel exposed with the oxide on as to copper. There were a number of other experiments, the general result of which was the same. He was considerably surprised when he read the paper. He knew that the experiments of the Boiler Committee had been continued, and he could only suppose that the author was not aware of what had happened. Mr. Barnaby had rather understated the case in favor of steel. In most of the cases that had been tested by the late Boiler Committee the results were strongly in favor of steel. Having a full knowledge of all the experiments conducted for the Admiralty for some years, his opinion was greatly at variance with that of the author as to the general result of the comparison between iron and steel. No one acquainted with the electrical effects of the combination of metals would expect to find any reliable inference from such a combination as had been presented. Copper, gun-metal, zinc, iron, steel, had all been combined electrically, and at various distances, some in contact, some supported on metal rods, some on iron, some on steel; and it was stated that the results were so much per cent. in favor of iron. For himself he would not undertake to say what the particular result was in any of those tests with any such combination.

Mr. John Donaldson could corroborate the remarks of Dr. Siemens and Mr. Barnaby, as to the evil influence of the presence of black oxide on steel plates, by mentioning two cases which had come under his notice. The practice of his firm, at the time when the boats were built, was to preserve, as far as possible, the oxide on the plates, with a view to prevent them from getting rusted, the oxide itself protecting the plate covered by it from rust. The first case was that that of a boat built for service on the west coast of Ireland. For some time

after leaving the yard they had most favorable accounts of her performance, but suddenly they received a letter, stating that she had one day gone out fishing, and it was as much as two men could do to keep her from sinking. The boat was examined, and it was found that the plates in the bow and in the stern, and others in the boiler and engine-room, were pitted with very small holes. It appeared as if some of the black oxide had been knocked off in the process of working the plates, and that the oxide left on had contributed to increase the rusting of those parts. At first he thought it was due to the engineer not clearing away the ashes from the stoke-hole, and the soot from under the smoke-box; but an examination of the second case, that of a boat built for the Zoological Station at Naples, seemed to show that it was more the action of the black oxide, helped largely, perhaps, by the plates not being kept well painted. In that case the principal pitting took place at the bow and stern, and scarcely any in the neighborhood of the engine-room. He therefore concluded that it was the black oxide that had been acting galvanically in oxidizing those parts of the plates that were exposed. When the plates of a boat were well painted and kept clean, very little of that action took place, as was shown in the case of the Lightning, which his firm built some years ago, and the bottom of which they had lately scraped and painted, the plates being found in excellent condition, due no doubt to the great care taken of the vessel while in the hands of the Admiralty. Their practice now was not only to remove the scale from the plates, but to galvanize the whole of the hulls, and since that had been done there had been no trouble as far as oxidation was concerned.

Sir Henry Bessemer was sure no one could help feeling that the question brought forward in the paper seriously affected a great deal that had been done within the last twenty years, and was well worthy of investigation. As a preliminary to his remarks he desired to say that he accepted unreservedly all the figures which had been given by the author; he accepted his experiments as stated, and had no doubt of their entire fairness. But if he were asked from those experiments to draw the inference that mild steel in the form of a boiler such as was ordinarily used would not endure half the amount of use and resist corrosion as well as common Staffordshire plate, and that phosphorus being present in the commoner irons, was the cause of its standing so well as compared with mild steel, he must entirely demur to any such conclusion. In the first place, the experiments on plates suspended inside a steam boiler appeared to him to be under totally different conditions from those to which a steam boiler was exposed when in use. A steam boiler in use had its internal flue exposed to a very high temperature on the inside, and on the outside of the water only, subject more or less to a deposit on its surface, and to a great many strains continuously by the raising and lowering of the temperature, which a suspended and insulated plate in the water did not feel or come in contact with. The external shell of the boiler also was exposed on the inside to the corrosive action of the water, and on the exterior to the corrosive action more or less of the gases of the furnace passing along the flues, and also to the escape of water occasionally from the weeping at the rivets, and so on, sometimes cutting large notches, or, if the boiler was well riveted, not affecting it at all. To these violent changes of temperature the suspended plate was not subjected. The deposition of scale upon the surface would not take place on the suspended plate in the same manner as in the case of a boiler. The conditions, therefore, were in reality very different, and that difference would sufficiently account for the fact that boilers in actual use did not corrode at the rapid rates that the suspended plates appeared to have done, from the evidence given in the paper. Some twenty-three years ago he manufactured a great many boiler-plates for a gentleman who was about to make experiments upon their use, and who desired to know what was the actual working condition of those new steel plates. In one instance 50 tons of plates were made into boilers, and the result, after twenty-two years' use, was very different from that which had been described with regard to the suspended plates, in consequence of the different conditions to which they were subjected. As intro-

ductory to the experiments, he might be permitted to refer to the remarks made by Mr. Daniel Adamson, M. Inst. C. E. (than whom there was no more thorough and practical man connected with boiler work), and by Mr. William Richardson. The occasion was the discussion on a paper which he had read on the 31st July, 1861, in Sheffield, before the Institution of Mechanical Engineers, Sir William Armstrong occupying the chair. Some specimens of flanging for locomotive boilers were then exhibited, and after they had been examined by the chairman, the remarks he had referred to were made in reply to an inquiry whether the plates from the new steel were much used. The plates of some of the boilers were exceedingly thin, and if anything like rapid corrosion took place in those plates, they would soon have become too weak to sustain the pressure of 85 lbs. to which they were subjected, and would have given way. It had occurred to him that it would be well to ascertain the condition of those boilers at the present moment, inasmuch as, if corrosion had gone on at the rate stated in the paper, in connection with some of the Bessemer steel, the plates would have been entirely destroyed in about eight and a half years, and as the boilers were made twenty-two years ago, it might be supposed that a second or third set would now have taken the place of the original ones. He accordingly sent the following telegram to Mr. Richardson: "A paper on corrosion of steel boilers will be discussed to-mor-morrow at Civil Engineers; are the six boilers of my steel, made twenty-two years ago, still in use?" To which Mr. Richardson replied: "The six boilers of your steel, made twenty-two years ago, are still in use, and have no appearance of corrosion." That was the best answer he could give to the assumption that Bessemer steel lasted about one-third the time of common Staffordshire plates. Had common Staffordshire plates been used twenty-two years ago he fancied that the boilers would be in a rather queer condition at the present time. Mr. Richardson's practice was to overhaul his boilers annually, and thoroughly investigate them; and as the boilers in question were experimental ones, and put up so long ago, he had no doubt that a strict examination had gone on in connection with them, so that Mr. Richardson would well know whether they were corroded or not. This statement was, he thought, the best evidence that could be given that Bessemer steel, and other mild steels of a similar character, were thoroughly well adapted for the manufacture of steam boilers, and might be relied upon quite as well as Staffordshire iron, notwithstanding the results arrived at by the author.

Mr. E. Matheson said the author had dwelt entirely upon boiler tubes and ships; but there were other matters in connection with the corrosion of steel and iron which were interesting to many members of the Institution; he referred to structures like bridges, exposed not to salt water or steam, but to the weather. The corrosion of wrought iron was very serious, especially in cities and in railway tunnels, and if steel were still more sensitive to corrosion, it would, indeed, be a grave matter. It would be interesting to compare the means taken to preserve wrought iron with those used in connection with steel, and to see how one would be suited for the other. He thought that the means taken to protect wrought iron from rust were imperfectly understood. There always appeared to be an attempt to preserve the skin of the iron as it left the rolling mill, and he ventured to say that that was impossible, and was to a large extent, a mistake. Wrought iron, in passing through the rolls, coming in contact with the atmosphere, was at once oxidized; it got a black scale or oxide upon it which must ultimately fall off; and although elaborate specifications were prepared for oiling or painting such wrought iron, that treatment only postponed the evil. Oiling wrought iron was, he thought, a better protection than painting, but even that did not fulfill the purpose intended. He believed there were only two, or perhaps three, modes of protecting wrought iron from rust. One was to keep it entirely from the air. Iron built into lime or brickwork or masonry would remain for centuries almost in the same condition as when it was put in. The second plan was that of Professor Barff, exposing the iron to superheated steam, but this was possible only with pieces of moderate size. A third plan was to remove the thin scale

entirely before the painting was applied, and he thought that was seldom, if ever, done in England in any sort of structure. The only place where he knew of its being done was in Holland, where the specifications of the engineers generally described, with the greatest minuteness, how the iron was to be treated before the oil and paint were applied. The iron was treated as the galvanizers treated it in this country; it was dipped in baths of dilute acid, which removed all the black scale. After washing it was painted, and if the paint was renewed from time to time, the iron might be permanently preserved. In this country the plan was followed necessarily by galvanizers, but he believed by no one else. If the structure had parts that were inaccessible to the painter's brush, rust would be sure to destroy it. He had lately seen a curious instance of the way in which rust deteriorated structures. He had taken down a beautifully-made bridge that had been put up twenty-five years ago by Messrs. Fox and Henderson; it was the first pin-bridge made in England, and was placed over the Commercial Road at Stepney. The upper boom or box of the bridge had been riveted and calked like a boiler, and was perfectly air-tight; and the inside plates, which had never been painted, were as good as the day when they were first put in, while some of the parts exposed to the atmosphere, and ineffectually painted, had deep pits bitten out in all directions, materially weakening them. The worst part was where the iron had been brought in contact with wood, the acid of which had so destroyed it that an angle iron $\frac{1}{2}$ inch thick was worn down to a knife edge. It was not often that one had the opportunity of dissecting an existing bridge, and he thought it might be interesting to mention the facts to which he had referred. Another bridge, an approach to a large terminus, put up some twenty years ago, was now being taken down (having to be widened) in the City of London; the rivet-heads were eaten away, and the T-iron stiffeners were nearly rusted away. At the present rate, in ten years he imagined the bridge would have begun to sink under its load. When he saw it there were four locomotives on it, so that it had to undergo severe strains. It would be interesting if some steel-maker or chemist would compare steel with cast iron. Like cast iron, it had been in a molten condition, and had not undergone the intermediary process of piling and laminating which wrought iron had to undergo. Cast iron, when run into a sand-mould, got a skin on it, which was a very valuable protection, and might be permanently preserved. Steel was cast in an ingot, and it might be useful to be informed, by those cognizant of such matters, how far steel cast in a sand mould was like iron cast in a sand mould; what difference there was on the surface of the steel, because it was cast in an iron ingot mould, instead of in sand, and what alteration took place in the steel by its being reheated and passed through the rolling mill. Apart from any chemical difference, there were these differences between iron and steel, and he felt sure there was a difference of surface caused by the way in which they were manufactured.

Professor F. A. Abel said the author had stated "that the commoner sorts of iron, containing the most phosphorus, resist corrosion far better than the superior kinds; and also that the harder steels, containing the greatest amount of carbon and phosphorus, are better in this respect than the softer and finer sorts." The author had further remarked that the conclusions at which he had arrived from the results of experiments had been confirmed by the recent analysis of some brands of metal under consideration. The subject being one of great interest to him, he had searched the paper, but in vain, for any facts upon which those conclusions were based. The author had very justly stated that "It is important to ascertain how far want of uniformity in composition has to do with local corrosion in metals, and particularly in steel; also how far the presence in a medium degree, or absence in a minimum degree, of impurities in iron and steel can affect their durability." Another statement made by the author was, "that the manufacturer and chemist, in their anxiety to produce a metal containing the least possible amount of impurities, and thus to attain a high standard of ductility, in depriving it, perhaps, to a greater degree than necessary, of elements such as phosphorus, carbon, &c.,

or adding to it manganese, probably thus render it more liable to corrosion." It was stated as a probability, but further on it was given as a decided fact; and he confessed that he was unable to conjecture how the probability had been converted into a matter of certainty. The author had made a general statement with regard to the proportions of phosphorus, carbon, and manganese in irons and steels. In regard to phosphorus, his argument might be to some extent borne out by that statement; with reference to carbon, the proportion in steel, according to his statement, was from two to three times that existing in wrought iron, and he had given the proportion of manganese as three or four times greater in steel than in iron. He imagined that the author wished to compare the two materials together, since, in the case of phosphorus, he had referred, not to the different proportions of phosphorus contained in one and the same material, but to the different proportions contained in the two materials. Those were the only facts bearing on the above conclusion which he had been able to find in the papers, and as, according to these, carbon and manganese were present in larger proportion in steel than in wrought iron, he should have imagined that they ought, according to the author's conclusions, to exert their protective influence to a far greater degree upon steel than they did upon wrought iron. That was the extent to which he had received instruction in reference to the impurities contained in wrought iron and steel, which the chemist was so anxious to remove; and he confessed the conclusion at which he had arrived was that, as regarded "assumptions and theories," the author did not stand upon much higher level than those whom he had condemned. The author had been very severe upon those who indulged in the view that possibly galvanic action might have something to do with the deterioration of boiler plates, whether iron or steel, but he thought there were very few persons, even among those who were not what the author had called indiscriminate partisans of mild steel, who shared that view, and who were not convinced that even in one and the same piece of plate galvanic action might come into play very decidedly in promoting the corrosion of the metal.

In reply to the possible objection that more definite results would have been obtained from more extensive trials, the author had stated that all experiments in which the conditions were not precisely similar, could not be considered satisfactory; and Professor Abel thought there were few who had experimented with a view to obtaining precision of results, who would not most heartily agree with him in that respect. But there, again, he could not quite understand how such very strong conclusions as the author had drawn could be based upon the results of experiments made by the distribution of large numbers of plates to different ships, in which they had received all possible kinds of treatment, especially when he found that the results of those various experiments with fifty-six sets of plates were lumped together afterwards, and that the conclusions were drawn from the average loss of weight of the plates in those remarkably various experiments. He was one of those who considered that among the most dangerously misleading figures with which practical and scientific men could have to deal were so-called averages. No doubt the results of the various experiments were interesting, as illustrating the character of the corrosion caused by different treatment, such as the water used, the different practices of blowing off, and the comparative effects of chemical agents. It was, however, from those various experiments that a definite conclusion was drawn with regard to the stability of steel as compared with iron, for the author stated, "These results clearly prove that the conclusions arrived at by many experienced engineers and chemists as to the causes of corrosion in boilers, previous to the appointment of the Boiler Committee, were erroneous." He should have been glad if a statement of those conclusions had been given, in order that their fallacy might have been demonstrated; for he should have been most anxious to learn from such an extensive and instructive series of experiments as the Boiler Committee had the power to institute, and did institute, to what extent previously existing conclusions were wrong. The author had also stated that "in spite of these reasonings, it is undoubtedly a fact, that under almost all circumstances iron, and particularly the

harder classes, is far superior to the finer steels in its resistance to corrosion, and this the experiments described by the author incontestably prove." That was a very strong statement, but he had no doubt that the author had considered himself perfectly justified in making it. He must, however. pardon those who were unable, from the summary of experiments which he had given, to arrive at the same conclusion. Taking even the averages upon which the author relied, this statement was not at all well carried out, for he could hardly consider that 10 per cent. difference in average loss would be so great in practical experiments as to constitute an "incontestable proof" of the difference between the durability of two descriptions of material. Further on he found that there was only a difference of 5 per cent. to establish the same point "incontestably." With regard to the averages of the experiments made on board the many ships, there were differences, in one case between crucible steel and Staffordshire iron, amounting to 0.3 per cent. only upon the weight of the total metal employed, and in another case between Siemens steel and Yorkshire iron of less than 1 per cent. upon the total weight. He could hardly accept results of that kind as "incontestable" proof that the superiority of iron over steel had been established. Again, in some of the special experiments made by the author, it had been "incontestably proved" that iron was on an equality with steel, and even that steel was superior to iron. Taking all these points into consideration, although he had read the paper with great interest, he could not admit that the author had in the least degree established, by the "facts" with which alone he proposed to deal, the "incontestable" superiority of wrought iron over steel as a material for boilers.

Mr. E. A. Cowper wished to say a few words in reference to the bridge to which Mr. Matheson had alluded. He had constructed it more than thirty years ago for the late firm of Fox, Henderson and Co. It was in the Commercial Road, and was 120 feet span, and had been lately pulled down to increase the width of the bridge. He believed it was the first bow-and-string bridge made of iron in this country; the bow flange was of a box form, with an overhanging top section. The span of the bridge did not require that the rib should be large enough to let a man pass through. There were vertical ties and large bolts which would prevent this; therefore the box was built as air-tight as it could be—not absolutely, but so close that the air could not work in and out with freedom. In that way the inside plates, he believed, were protected very fairly, until it had been lately pulled down. Mr. Matheson had not said whether the inside plates were in a perfect condition, but he believed that they were so. In some of the outside plates, where wood had been placed against the side of the box, the iron was corroded, and in some cases very badly, no doubt from the tannic or gallic acid in the wood. But where bridges were made with laminated plates, as they were in many instances—notably in one near at hand where corrosion was going on rapidly—he imagined that in a few years there would be no plates left. It was impossible, with a number of plates riveted together, to get them all so air-tight at the edges as entirely to exclude the atmosphere, and especially the damp air coming from the chimneys of locomotives. The mode of protection suggested by Mr. Matheson was no doubt a good one, but he would suggest that a further protection should be adopted by forcing varnish or boiled oil, or whatever might be the best material, between the plates by a powerful syringe; the oil might not go all the way in, but it would go as far as the air would, and two or three injections might entirely stop the entrance of air. In that way he believed that many bridges of laminated plates which were now in a state of deterioration might be preserved from further deterioration between the plates. Thirty years was too short a life for a bridge, and some improved method of protection ought to be adopted.

Mr. Peter Samson remarked that the paper was bristling with facts which were exceedingly interesting and useful to Engineers, especially to those who were entrusted with the maintenance of such structures as bridges, ships and boilers. At the same time engineers ought to be exceedingly cautious in drawing deductions from those experiments, otherwise they might, without ground, get alarmed and scared from the use of mild steel.

He thought it would have added to the value of the paper if the loss from corrosion had been given per unit of time as well as per unit of surface, because it was impossible to compare one experiment with another, or to compare the experiments with the actual corrosion going on in boilers unless that were done. He had calculated the loss of weight of the iron plates as given in the tables per square foot of surface per month, and he had found that it averaged 55.6 grains, the greatest loss being 170 grains, and the lowest 10.9 grains. That corrosion, although it might appear large, was small in amount when compared with the active corrosion going on in some boilers. Taking the average corrosion of the experimental plates as a standard by which to arrive at the durability of a ½-inch plate, 1 foot square, if corroded on one side only, he found that it would take more than two hundred years to corrode the plate entirely away. Comparing this with the average life of a well-kept marine boiler, which was only about ten years—although it was not an uncommon thing for a ½-inch plate in a boiler to be corroded away in a very few years—it was evident that the conditions under which the experiments were made, and those actually going on in boilers were entirely different; and the question arose, what effect the difference of conditions had on the results of the experiments? It was important to note that the percentage in favor of iron was 45.4 in the case of the tubes referred to in Table II., and only 10.9 in the case of the feed-water heater plates, where the rate of corrosion was about 70 per cent. greater. It would be exceedingly interesting to know the author's views as regarded the corrosion being so little in the experimental plates compared with that in actual boilers. Referring to the experiments which the author had made at home, he thought those results would be found useful and interesting, but they required some explanation. In those referred to as set 98 the corrosion in the first twelve months was considerably higher than that during the second twelve months, and as there was no apparent reason why it should be so, especially as the plates must have been scraped and cleaned for weighing at the end of the first period, he thought it would be useful to have the author's explanation on that point. It would also be interesting to have his opinion as to why the corrosion in the experiments marked 102 was so much less than in those marked 98. Possibly it might be due to the water in the former series being kept in a vessel without being changed, and if that was the case it would tend to prove that the water in boilers should be blown out as seldom as possible. Referring to the experiments with the crucible steel, it would be found that the plate which was placed in the water-butt corroded more than that placed in the tank, and that the plate in the kitchen boiler corroded less than the plate in the tank. The difference might have arisen in this way: in the water-butt, the corrosive agents were very active, owing to their not having previously come in contact with a substance for which they had an affinity, and combined freely with the experimental plates suspended therein. They were then carried on to the tank, where the second set of plates were placed, but being weakened by their first attack, less corrosion occurred, and they finally passed into the kitchen boiler weaker still, and therefore less able to combine with the last set of plates.

Mr. D. Phillips in reply observed that his sole purpose in writing the paper had been to lay before the Institution facts which had come to his knowledge regarding the comparative corrosion of iron and mild steel. He had no wish to depreciate mild steel. True, the paper showed that that metal had defects; but these, he was of opinion, could be counteracted with further knowledge. It would not do the cause of mild steel any good to conceal its faults, for without clear knowledge of the defects of steel it would be impossible to remedy them. Many of the criticisms had been wide of the mark. Whilst the paper dealt chiefly with the corrosion of iron and steel having bright surfaces, considerable time had been spent in discussing the mechanical qualities of steel, and its liability to corrode with or without black or magnetic oxide.

He could not but admire the convenient way in which Dr. Siemens had attempted to prove from the figures in the paper that its conclusions were erroneous. Dismissing the tube and disk experiments as worthless, and skipping the experiments in the two tugs and the feed-

water heater, and those with Lowmoor iron and Bolton steel, he observed that in the test of the Landore steel and the Lowmoor iron, the latter proved only 4.8 per cent. better than steel; but this was in fresh water, and only for six months. Referring to Table VII. and the remarks following, he said that in this case the iron lost more than twice as much as the steel; but then the iron mentioned as having been tested in a former experiment was under trial over twice the time that the steel was. Ignoring the experiments in ocean and coast-going steamers, perhaps the most important of all to those engaged in steam-shipping, Dr. Siemens proceeded to pick out from Table IX. such results as suited his purpose. In set 98, he compared the Landore steel with the Yorkshire iron, whilst in set 99 he compared it with Staffordshire iron. Passing over sets 100 and 101, in which both the steels gave bad results, he wound up with set 102. Now this was hardly an impartial way of treating the matter. To take into consideration only such facts as confirmed one's own opinions was a species of special pleading upon which Dr. Siemens could not be congratulated. In sets 99 and 102 the corrosion was scarcely perceptible. The D.D. 102, to which Dr. Siemens drew attention, had a patch of cinder in it, which was picked out before testing, but after it was weighed; the loss of weight should therefore have been approximately reduced. In calculating the percentages in Tables VIII. and IX., the two steels and the two irons were compared, the difference between each steel being but trifling. The results obtained, after a little over two years' trial, from the metals tested in the tube apparatus, were considered by Dr. Siemens to be valueless; but when he was better acquainted with the conditions to which the apparatus and the tubes had been subjected, this opinion would probably be modified. Mr. Phillips possessed, from a scientific point of view, only a scanty knowledge of the effects of electricity on metals, but he claimed to have a fair amount of knowledge as to the nature of corrosion in marine boilers and its causes, and he failed to see why, in the tube experiments, with metals so closely allied to each other, one metal should feel the effect of electricity more than another. He thought it would be admitted that neither in the steam chamber nor in the condensing chamber were there present conditions such as would promote galvanic action between the iron of the apparatus and the tubes, or between the tubes themselves, nor after the experiment was any such action to be perceived. At, and a little above, the water level in the condensing chamber the tubes suffered severely, but below and above that point there was scarcely any corrosion. Again, in the steam chamber the surfaces, especially of the cold-drawn tubes, were scarcely affected. There were no disks or rods in any of these tubes. The conditions necessary to promote galvanic action inside the tubes were also absent, except, perhaps, in set 2, which contained salt water. The other three sets contained fresh water. In set 2, galvanic action could only take place between the iron plugs and the bottom ends of the tubes, but there were no signs of such action. If, as Dr. Siemens remarked, the common iron alone was compared with the Bessemer steel tubes, the difference was nearly 200, not 300, per cent. in favor of the iron; but in Table II. the losses in all the tubes were given, and in Table III. the losses in ten samples of each metal, under precisely similar conditions. In calculating the percentages resulting from them, he had given the true results. The tube apparatus had been designed by him at the suggestion of the Chairman of the Boiler Committee, Admiral C. Murray Aynesley, for testing tubes of various kinds of metals. Mr. Farquharson had nothing to do with it, and his remarks as to magnetic oxide showed that he did not understand the conditions under which the tubes had been tested.

Mr. Phillips could not understand Dr. Siemens when he said "there were substances, compounds of iron, manganese, and silicon, sold for steel, which no doubt corroded very rapidly." The metal apparently referred to by Dr. Siemens was made by the Bolton Steel and Iron Company, on the Bessemer principle, and was considered by most practical men to be as good, in every way, as Landore steel. The weighing of the specimens in the experiments had been carefully done, chiefly by Mr. Tookey, one of the members of the first commit-

tee, and by Mr. Ireland, one of the members of the second committee, every plate, disk, or tube having been separately weighed.

As regarded the effect of magnetic oxide on steel, Dr. Siemens did not make it clear whether iron suffered from its oxide in the same manner and proportion as steel was said to do, nor as to the conditions under which this action took place. From what Mr. Phillips had been able to gather in this direction, the oxide could only affect a metal to which it was partially attached in salt water at ordinary temperatures, its effect in cold fresh water, and in fresh and sea water of high temperatures, such as in marine boilers being absolutely nil. Further, every practical engineer knew only too well that this magnetic oxide in a marine boiler, especially below the level of the water, soon disappeared, much sooner, indeed, than could be wished. With the exception of the small plates and the welded tubes, all the specimens in the experiments were effectually freed of their oxides, either by planing, filing, or grinding, so that the remarks of some of the speakers as to the black oxide were hardly to the point. According to Dr. Siemens, the fact, if fact it were, that black oxide materially assisted the corrosion of steel, was a recent discovery of Mr. Barnaby, or of one of his staff, but not in connection with the working of marine boilers. Professor Williamson had, however, pointed it out to the committee in 1874. It might prove of value to shipowners and builders, but if not it would not want companions in the limbo of neglected discoveries. The first volume issued by the Boiler Committee was full of theories as to galvanic action, which, with the better knowledge now existing of the working of boilers, were considered unworthy of serious consideration.

The remarks of Mr. Traill and Mr. Martell contained nothing new and nothing to comment upon, except, perhaps, the instance of extraordinary corrosion mentioned by the latter. Mr. Martell, attributed this, as many others would do, to galvanic action between the iron rivets and the steel plates. Had the action taken place below the light water-mark, there would be some reason for attributing it to galvanic action, but in his opinion, had the rivets been of the same material as the plates, a similar corrosion would have taken place. Mr. Martell had remarked, in May, 1879, "that it was not uncommon to find soft iron rivet points in iron ships somewhat pitted or worn within the surface, but the converse action of the plate wearing around the rivets was peculiar." This seemed to confirm Mr. Martell's view that the softer or purer metals did not resist corrosion so well as the harder sorts, though the action would, in such cases, be assisted by a sort of breathing or springing of the plates in a line with the rivets, especially if the plates were thinner than they would have been if of iron, by which, and friction, the paint was soon removed. Mr. Martell had no doubt often seen the sides of iron ships corroded similarly, though not to such a serious extent, after a long voyage in the tropics; he had himself seen many, amongst others two old iron hulks in Bombay harbor, completely riddled, a little above the water line from the effect now of air, now of salt water and want of attention. According, however, to the experience of Messrs. Martell, Barnaby, and Donaldson, deterioration went on in steel hulls which never occurred in iron hulls. In one case this was attributed to galvanic action between the iron rivets and the steel plates; in the other two cases to galvanic action between the steel plates and their oxides. Mr. Barnaby got over the difficulty by removing the magnetic oxide by pickling the plates, and Mr. Donaldson by galvanizing them with zinc. Why should this be necessary with steel hulls, and not with iron hulls? Practical men, he thought, would pause before they adopted either plan, and would consider how it was that the oxide on iron plates did not produce the same effect as that on steel plates.

It had been mentioned by Mr. Barnaby, not for the first time, that the results of some experiments carried out by the Admiralty contradicted those given in the paper. In Paris, in 1878, Mr. Barnaby said that the Admiralty "had made some experiments extending over four years, and although at first the pieces of steel suffered more loss than the pieces of iron, when they were put unpainted on the bottom of the ship in salt water, as times went on they discovered that the loss did not continue, and that, so far as these

experiments went, steel was at least as good, and it appeared to him to be better, than iron." In London, in 1879, he said "in the matter of oxidation in salt water, we have found by a series of trials extending over about three and a half years, that the rate of oxidation of three plates of iron of the same brand, differed more among themselves than they differed from steel; that when the surfaces of steel plates are carefully freed from the black oxide produced in the rolls by a wash of weak acid, or otherwise, the surface corrosion in salt water is very uniform; that when the surface oxide is left on, the effect of the oxide on the neighboring bared metal is as strong and continuous as copper would be." Now, he was curious to know what experiments Mr. Barnaby referred to. Experiments had been made by the Admiralty with iron and steel plates at Portsmouth and Devonport, between 1874 and 1877, to test their comparative durability in sea water and in bilge water. The specimens tested and the results obtained came under the notice of Admiral Aynsley, Mr. Tookey, and himself; they ascertained that the steel plates tried at Portsmouth lost 80 ozs. 341 grains, and the iron plates 61 ozs. 52 grains, or 24 per cent less. In the experiment at Devonport, which lasted a much shorter time than that at Portsmouth, the steel plates lost 43½ ozs., the iron plates 26½ ozs., or 64 per cent. in favor of the iron. The dockyard officers reported that the corrosion in the steel plates was more severe and irregular than in the iron. Surely these experiments were not those to which Mr. Barnaby alluded. In a letter addressed to the Controller of the Navy, dated the 6th of June, 1877, the committee pointed out the confirmation the results obtained at Portsmouth gave to their own conclusions. He would ask, too, why these results, and those of other experiments since carried out by the Constructor's Department, had not been made known? In such an important question the Mercantile Marine was as much interested as the Admiralty.

Some time had been devoted by Sir Henry Bessemer to show the different conditions to which plates suspended in boilers, and boilers themselves, were subjected. That was hardly necessary. One of the objects of the committee's experiments was to ascertain the comparative endurance, as regarded corrosion, of different metals when under similar conditions, and this object they had attained. Sir Henry Bessemer had quoted Messrs. Richardson and Adamson, and it was always interesting to hear the experience of such practical men. But these gentlemen had to deal with land boilers, worked with water comparatively harmless to the materials of which they were composed. Mr. Adamson had been principally concerned with the construction of boilers; and he would ask why the results of the experience of these gentlemen, or of others, with iron boilers and steel marine boilers, had not been given? Mention had also been made by Sir Henry Bessemer of some steel boilers at Oldham, which had lasted twenty-two years, and which were now said to show no signs of corrosion, though this last remark could hardly have been seriously meant, and he opined that had these boilers been made twenty-two years ago of Staffordshire iron they would now have been in a very dilapidated condition. When the Boiler Committee visited Oldham in November, 1874, Mr. Richardson was engaged in removing and resetting iron boilers which had already done duty for more than eighteen years, and which were, he had been told, still at work. The shells of these boilers were, as usual, of ordinary iron. Again, at Wigan, in 1875, he had seen some iron boilers which were more than thirty-one years old, working at pressures of from 60 lbs. to 70 lbs. to the square inch, the front ends of the flues only, over the fires, having been renewed; these, he was informed, were still at work. The following was a copy of a letter, dated 7th April, 1881, which he had received from Mr. Mason, Superintendent Engineer of the Furness Railways: "Dear sir, replying to your favor of yesterday's date, the boiler of Firefly, paddle steamer, Lake Windermere, was put in in 1848, that of the Dragonfly in 1850; both were broken up last year. Plates (internally) good, having the bloom still on them. Boilers of Walney were put in in 1868, taken out in 1878. Steel boiler completely done. Iron boiler might have lasted three or four years longer with care. Yours, &c., R. Mason." This vessel had since been fitted with two iron boilers. In 1876 he brought

with him from one of the boilers on Lake Windermere a piece of iron tube which had been in use rather more than eighteen years. On the water-side of the tubes, furnaces, &c., there was very little corrosion, but the surfaces exposed to the action of fire, sulphur and the atmosphere, had suffered a great deal, especially round the lower part of the fronts. Here were cases of iron boilers lasting over thirty years on board steamers, as well as on land.

It had been remarked by Mr. Ravenhill that if steel wasted as quickly as the specimens in set 100, "there ought now to be little of the original steel left" of the Dover boats. These boats made runs of about one hour and three-quarters' duration, allowing of their sides being examined and painted daily, if necessary, whilst the set of plates (100) were under a very severe test, and not protected—not even by their oxides; both sides of the experimental plates were wetted daily and exposed to the weather, and it was only to be supposed that they would suffer more from corrosion than plates in ships' sides painted and taken care of. He could not admit that the report received by Mr. Ravenhill from Captain Dent gave such evidence as was desirable in these matters. Indeed, he had heard that the steel boilers in the Duke of Sutherland, one of the Holyhead boats, had suffered more than the iron boilers.

With regard to Mr. Matheson's remarks, if iron and steel were kept dry, or well looked after and painted, they would suffer but little, if any, corrosion; also with care the magnetic oxide would remain attached to the surfaces of iron structures, under ordinary conditions, for years, and would, while it remained, effectually retard corrosion.

In reply to Professor Abel he contended that the results of the experiments he had described confirmed his conclusions. If the method of giving averages were unfair, the gross results would still uphold this view. He would refer any one desiring fuller information concerning the ocean plate and tube experiments, and the experiments in the tugs, &c., to the Blue Book just issued by the second Admiralty Boiler Committee. Professor Abel went on to say that since there was more carbon and manganese in steel than in iron, these elements should afford steel greater protection than iron. Carbon undoubtedly did so, but the part manganese played in this respect was doubtful, probably the reverse; taking, however, all the so-called impurities in iron and steel together, there was a greater sum total of foreign matter in common iron than in mild steel, and these foreign impurities or ingredients might be the cause of common iron corroding less than mild steel. Professor Abel said that he stated this as a fact; that was not so; he stated as a fact that the metals which contained most impurities, especially those containing the most phosphorus, resisted corrosion better than the more refined sorts. More than one scientific man supported the views he had advanced as to the class of metal which suffered most from corrosion; and Dr. Frankland held the same opinion as himself regarding galvanic action in boilers. He would refer to the summary of the evidence given before the Boiler Committee in 1874. by such authorities as the late Dr. Letheby, Dr. A. W. Williamson, Dr. E. Frankland, Dr. J. Percy, Mr. G. J. Snelus, and Mr. W. Weston, and from speeches made at the meetings of the Iron and Steel Institute in 1878-9, by Mr. I. Lothian Bell, Dr. Siemens, and Mr. E. Riley, and it would be seen what different conclusions such gentlemen came to. He regretted that Professor Abel should have confined his remarks to criticism without endeavoring to throw any light on the subject of the paper.

Two of the experiments described by Mr. Phillips showed only 5 and 10 per cent. in favor of the iron, and this it was said was not "proof incontestable," that iron was superior to steel as regarded corrosion. If even these percentages were added to the 13, 20, or 25 per cent. reduction in the sectional area of steel plates and scantlings in ships or boilers accepted by the Board of Trade and by Lloyd's, which matter had been conveniently forgotten in the discussion, or if the corrosion were assumed to be equal in the metals, the result would be, if not fatal, very damaging for steel.

The greatest care was taken that the specimens in each experiment should be exposed to exactly the same treatment. With regard to the ocean-plate experi-

ments, when fifty-six sets of test plates had been subjected to the various sorts of treatment of boilers now in practice, and furnished a percentage of 21.3 in favor of the irons over the mild steels, it was not only fair, but reasonable, to infer that iron on the whole withstood the corrosive effects of those treatments better than steel. That the treatment of boilers might be so modified as to alter this state of things he had already suggested, and indeed had often put practically to the proof.

In reply to Mr. Samson's queries regarding the results of the experiments given in Table IX., the greater corrosion in sets 98 and 100 during the first period of their trial than in the second period, was due in his opinion to the weather in the summer of 1879 being so much more changeable than in 1880. Mr. Samson had furnished the reply he should have given as to the greater corrosion in set 98 than in set 102, viz., that the vessel containing set 98 was much larger than that containing set 102, and that the water in it was constantly changed. The tank and boiler containing the crucible steel were small, and the water in the boiler was kept at a temperature of from 120° to 212° during about thirteen hours daily, which would account for the difference in the corrosion in the three plates.

It had been calculated by Mr. Samson from some of the experiments described in the paper, that it would take two hundred years to corrode boilers entirely away; several cases were mentioned in the Blue Book in the ocean-plate experiments, when a $\frac{5}{8}$-inch plate would be entirely gone in from two and a half to five years, while other plates would last more than thirty years, both sides of the plates being exposed to the water in the boilers. In other instances extraordinary differences would be found as regarded the amount of corrosion sustained by one metal compared with others of the same set, besides those given in the paper. In set 24 the loss of the steels was more than twice that of the irons; in set 32 more than half as much again; in set 33 the loss of the Landore steel was more than twice that of the Bolton steels; in set 66 the steels lost nearly three times, and the Lowmoor iron nearly twice as much as the Staffordshire iron.

He had been informed that the experiments carried out and concluded some time ago by Mr. Parker, Chief Engineer Surveyor to Lloyd's, confirmed the results given in the paper, the percentage in favor of the irons, taking all the results, being over 19, and in favor of the rough over the bright specimens 12. It was to be regretted that the conditions and results of these experiments in detail were not made known to those interested immediately they were obtained.

In conclusion he would ask why it was that iron tubes had been nearly always substituted for steel tubes in steel boilers, and why iron tubes were now put into almost every boiler, even though made otherwise of steel? Why the Admiralty should now be going back (as stated by Mr. Barnaby) to the use of iron furnaces and combustion chambers, whilst making boiler shells of steel? And why composite boilers of iron and steel should be made at all, especially by the Admiralty, after Mr. Farquharson's discovery as to the effect of magnetic oxide on steel, and the galvanic action which was asserted to be set up between iron and steel? Neither Mr. Farquharson nor Dr. Siemens threw light on the effect, if any, of this oxide on iron, nor as to the action between iron and steel, nor between these metals and their oxides at high temperatures in marine boilers. If Mr. Farquharson's discovery, or the theories of galvanic action were to be relied on, there would by this time have been very little steel left in the composite boilers made by the Admiralty and mentioned in the paper. He believed that when the question as to the effect of magnetic oxide had been more practically considered, it would be found that the local corrosion was not due in iron or steel to galvanic action, but to other causes, frequently to perfect protection being afforded, for a time, to parts of the surface by the now much abused oxide, the adjoining parts being unprotected, or to want of homogeneity, or uniformity in composition, or both. When portions of surface were so protected as to be comparatively unaffected, the difference between them and the adjoining affected parts was most pointed, and to unpractised eyes very deceptive. Some of the longest-lived marine boilers had either brass tubes or back copper tube plates, or both.

TESTING MACHINES, THEIR HISTORY, CONSTRUCTION AND USE.

By ARTHUR V. ABBOTT.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

I.

HISTORY.

To a celestial visitor, approaching the globe toward the Western Hemisphere, the American Continent would appear like the web of some gigantic arachnid. Long lines of glistening black threads, running from point to point, and here and there converging to a focus, would, on a nearer examination, be decomposed into parallel lines of railway, with accompanying telegraph wires. Spider-like bridges of wood and iron span the rivers, while over the waters, under them, glide all sorts of naval structures, bearing the commerce of a nation. A still closer inspection would decompose the foci of the railways into busy towns and cities, whose substantial buildings, of wood and stone and iron, bear testimony to the truth of the saying; that the prosperity of a nation may be measured by the success of the people as constructors. Thus, to conquer time and space by joining the two oceans with the railway and the telegraph; to drive a five thousand ton ship across the Atlantic in the face of adverse gales; to erect the structures that now span the Mississippi, the Niagara and the East River; and to dot the continent with buildings, such as may be seen in the streets of any of our cities; would seem to require the most intimate conceivable knowledge of all the properties of all the materials used in construction. Yet such is far from being the case. Indeed the art of construction, perhaps more than all others, is involved in mystery and obscurity; unless the fact that engineers, having arrived at a point where they are ready to say that they do not know, may be credited with having gained a long stride toward perfect knowledge. This unacquaintance with the subject is by no means unaccountable, for a little consideration will show that it is one of the greatest complexity. In designing an iron bridge, the engineer is at once brought face to face with such a catalogue of operative causes affecting the quality of the material to be used, each one having the power to vary so greatly the iron, that only the keenest foresight, and the most subtle analysis, can predict the result. A slight variation in the ore changing the relative proportion of carbon, phosphorus or sulphur, may give rise to the greatest differences in the refined metal. Changes of manipulation in the rolling mill, apparently slight, such as differences in the temperature at the time of rolling, or variation in the rapidity of the reduction of the rolls, are capable of enhancing or spoiling the value of the product. The knowledge of materials is at present, at least, an absolutely empirical one, for those who are most skilled do not hesitate to admit that any predictions from existing data regarding a new material, or the effects of a new process on an old one, are hazardous in the extreme, and should be received with the greatest caution. "Experimenta docet" seems to be the only motto for this science. Before the constructor makes use of either a new material, or an old one in a new form, the only safe method is to experiment with the piece in question, carrying the researches far enough to demonstrate all the physical properties, so that the qualifications for the work in question may be accurately known. Thus, and thus only, can the engineer inspire confidence in the structures that he plans, and by a knowledge superior to that of his competitors exhibit such an economy and fitness of design as shall ensure success.

To make experiments on large pieces of iron and steel, having a tenacity of from fifty to one hundred thousand pounds per square inch of section, requires the use of very expensive and ponderous machinery, to which may be at-

tributed the slow progress that has attended investigations of this character. The entire history of tests, and testing machines in this country may be comprised within the last thirty years. One of the first machines for making physical tests was designed and built by the late Major Wade, for the United States Government, in 1855 and 1856, and was used in making experiments on the cast iron intended for the ordnance service. A little later this machine was remodeled and improved by Capt. Rodman. Two of the Rodman machines are now in use by the Government, one being in the Washington Navy Yard, and one in the Army Building New York City. These machines consisted of a heavy frame of cast iron carrying three levers, the last and smallest one being used as a scale beam. One end of the specimen (which was turned so as to have two collars on each end) was secured to the iron frame, and the other end to the large lever of the scale system. This whole scale system was then lifted by means of a screw and compound gearing, thus producing stress on the specimen, which was estimated by placing weights on the scale beam. Cotemporaneously, the late John A. Roebling was engaged in making experiments on the properties of wire at his works in Trenton, and investigating the qualities of rolled iron beams at the works of the New Jersey Iron and Steel Co. A little later Geo. W. Plympton, now Professor of Physical Science in the Brooklyn Polytechnic, made numerous experiments on some full-sized rods designed to be employed in bridges built by Murphy & Whipple. This was one of the first testing machines built to make experiments on full-sized bridge members, and was of unusual dimensions. It consisted of a heavy framework composed of yellow pine timbers 16 ins. by 20 ins., and thirty feet long, set 18 ins. apart, and secured against side springing by the pieces of oak. The stresses were applied by a screw, three ins. in diameter, working through a thick iron plate, and one of the oak end pieces. The outer end of the screw was furnished with a rachet wheel 12 ins. in diameter, which worked on the extended screw as a fulcrum. A stout rachet on the lever completed the movable outside portion on the head of the machine. Inside the head-piece the screw terminated in a stout cast iron cross-head, in which it turned freely. The cross-head carried a pair of parallel flat bars to which the rod under test was made fast. To facilitate the fastening, and to render the machine adjustable for testing rods of different lengths, the parallel bars were furnished with holes 6 ins. apart, and a sliding cross-head that could be attached to the bars by means of pins placed in these holes. At the other end of the machine the stresses were measured by a balance beam 10 ft. long. A heavy iron bolt carried through the head-piece was (after being passed through a thick iron plate which formed the end of the balance beam) forked so as to hold securely a steel block, with a face at right angles to the axis of the rod; this face was made to bear against a knife edge when the machine was in operation. The balance beam was constructed like a king-post truss, with an extra rod following the line of the inclined studs. At the end next to the machine the plates above referred to were secured by bolts so as to form the extreme end of the lever. The plate had two blunt knife edges, one on each side; the one toward the machine rested on a plate attached to the oak head-piece; the knife edge on the other side, which was $\frac{1}{10}$ of a foot higher up, had for a bearing, the steel block which received the stress from the rod under test. To the extremity of the balance beam was hung a platform to receive weights. It will be seen that the tensile forces were measured by a bent lever, whose arms were respectively $\frac{1}{10}$ of a ft. and 10 ft. The machine was designed and built by Mr. J. W. Murphy of Philadelphia.

The outbreak of the war diverted the thought of the country to other channels, and little more was heard of testing machines till shortly after its close, when the scale apparatus of a large machine was built for Colt's armory by Fairbanks & Co. This machine is illustrated in Fig. 1, and had a capacity of 100,000 lbs. It has a platform *a*, with a central opening, through which pass two screws, *b*, entering the cross-head *c*, and connected at their lower ends to two arms of a forked lever below the floor. The long arm of this lever is coupled with the differential levers *d*, *e*, *f*, which act on the scale beam in such a manner that by depressing the free end

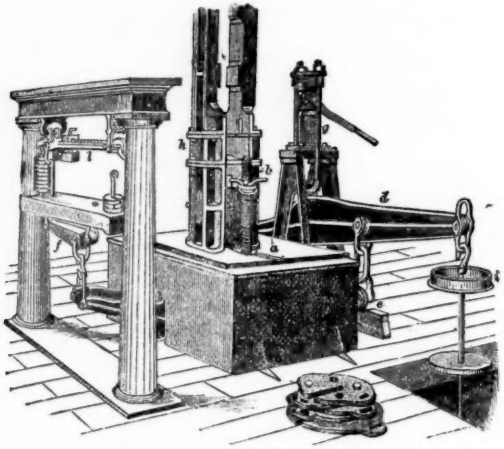


FIG. 1.

of the lever *d*, the cross-head is pulled down. This same effect may be produced by raising the fulcrum of the lever with the hydraulic jack *g*. The machine was fitted with appliances to make test in tension, compression and transverse stress.

This was the first *platform* testing machines ever built.

cross-head for holding the upper end of the specimen. Through the platform run two screws operating the lower cross-head, and moved by the gearing and hand wheel at the side of the machine. The force produced by the screws, whether in tension, compression or transverse stress, is brought on the

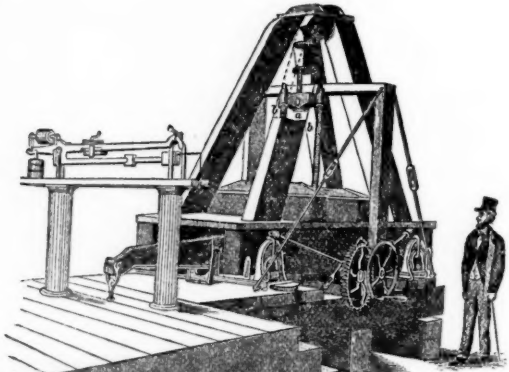


FIG. 2.

Another machine was soon built by Fairbanks & Co. for Columbia College, New York. Fig. 2 presents a view of this machine. A large platform supports four heavy timber columns that carry a top platform and estimated by the beam. This machine is fitted with a clockwork attachment for automatically moving out the poise on the beam as fast as the stress increases in the test piece. From

these machines as a starting point, Fairbanks & Co. have continued with constant improvements the manufacture of testing machines. Much interest was now manifested in the subject of testing, and the firm of Riehle Bros., of Philadelphia, started in the manufacture of all kinds of machines as regular articles of commerce.

At a convention of the American Society of Civil Engineers held at Chicago, June 5, 1872, it was, on motion of Gen. Sooy Smith, resolved, that:

Whereas, American engineers are now mainly dependent upon formulæ for the calculation of strength of the different forms of iron and steel not based on experiments upon American materials and manufacture; and, whereas, these differ greatly in many of their characteristics from those of foreign production, both in their nature and form: therefore,

Resolved, that a committee of five be appointed to urge upon the United States Government the importance of a thorough and complete series of tests of American iron and steel, and of the great value of the formulæ to be deduced from such experiments.

Pursuant to this resolution a committee was appointed by whose efforts Congress was induced to pass a law for the appointment of a United States board to test iron and steel, and an appropriation of 75,000 dollars was made for that purpose. This board consisted of Col. T. T. S. Laidley, Ord. Dept. U. S. A.; Commander L. A. Beardslee, U. S. N.; Lieut-Col. Q. A. Gillmore, U. S. A.; Chief Engineer D. Smith, U.S.N.; Gen. W. Sooy Smith, C. E.; A. L. Holley, C. E., and R. H. Thurston, A. M. C. E., Secretary. One of the first steps of the board was to secure an adequate machine for making the intended tests; for at that time there were no machines in the country capable of making accurate experiments on specimens of more than a square inch of steel. After a thorough investigation of the subject a contract to build a machine having a capacity of 1,000,000 lbs. was awarded to Mr. A. H. Emery, of Chicopee, Mass. During the four years that elapsed prior to the completion of the machine the board did a very large amount of very valuable work in investigating with the Rodman machine, and the large chain-testing machine at the Washington navy yard, the character of wrought iron rods commonly used for chains; and under the direction of Prof. Thurston, at Stevens Institute, the properties of the different bronzes. In 1879 the Emery machine was completed, and the government came into possession of a machine having a capacity of 100,000,000 lbs., and capable of testing specimens (in tension and compression) up to 30 ft. of length. Unfortunately the act incorporating the board limited its life to the duration appropriation of 75,000. With the completion of the testing machine this appropriation was exhausted and the board ceased to exist without having had an opportunity to demonstrate the value of the new testing machine to the community.

At the Centennial Exhibition in 1876, Professor Thurston exhibited a testing machine for making experiments in torsion, which was provided with an apparatus whereby the amount of the stress applied to the specimen and the resulting strain were graphically recorded by the machine on a sheet of cross-section paper. This was the first testing machine arranged to autographically record the results produced by the experiments, and marks a great step in the improvement of such apparatus.

In 1877, in connection with the inspection of the steel intended for the superstructure of the East River bridge, the author of this essay designed, and constructed what is believed to have been the first testing machine for autographically recording the results of the tests made in other than torsional stresses. This machine was not a large one, having only a capacity of 100,000 lbs., and susceptible of experimenting on pieces in tension, compression and transverse strains up to 2 ft. in length, and in shearing to capacity of machine. Briefly described, it consisted of a cast iron base supporting two cast iron columns, on which was placed a differential scale beam. The end of this beam was connected with a swinging weight, so that the stress produced on the specimen caused the end of the beam to rise, swinging the weight through a measurable arc. Connected with the scale beam was a cylinder, carrying a sheet of cross-section paper, over which was mounted a little carriage, sustaining a pencil or stylographic pen. This carriage was caused to move along the axis of the cylinder by means

of a steel tape, connected with the specimen under examination. As fast as the stress was applied to the specimen, the scale beam rose, the cylinder rotated, and caused the pencil to make a mark, parallel to the axis of y, while as fast as the stress produced any deformation in the specimen, the pencil was drawn along the paper parallel to the axis of x. The combination of these two movements gave a curve, whose abscissae and ordinates indicated the amount of stress and strain produced in the specimen.

About two years ago Messrs. Fairbanks & Co. conceived the idea of establishing, in New York City, a physical laboratory completely equipped with testing machines of the most approved pattern, and supplied with such gauges, and instruments for making all kinds of physical investigations, so that engineers, and those interested in construction, could be afforded an ample opportunity to make investigation in any desired direction. The many improvements in testing machines, now to be described, had their origin in the desire of that firm to equip their "Department of Tests and Experiments" so completely that it should be speedily accepted as a standard of scientific accuracy, and should become to America what the laboratory of Kirkaldy has been to England.

Scientifically speaking, a testing machine is a piece of apparatus for the purpose of breaking samples of material, and registering the amount of stress required, as well as the strain produced thereby; and an analytical consideration of the subject divides testing machines into two general classes, namely, machines for special tests and those for general work. Each of these classes may again be subdivided, according to the position that the specimen occupies into horizontal and vertical machines.

With regard to the method of applying the stress, testing machines may be classified as screw or hydraulic machines. While having reference to the method employed for measuring the stress applied, they may be classed as lever, hydraulic scale or hydraulic gauge machines. These several classes will now be treated in their respective order.

GENERAL TESTING MACHINES.

The machines under this class are those so planned as to make experiments on material in at least three or four different ways, as for example: machines arranged to make tests in tension, compression, transverse stress, and in shearing, bulging, punching and torsion. There are, however, very few machines in the country that aim to make tests in all of the above varieties of stress. Generally speaking, the investigations have been confined to tests made in tension, compression, transverse and shearing stress. While the appliances for torsion, bulging and punching are less frequently added. The classifications into horizontal and vertical machines, simply depends upon the position that the specimen has with reference to the machine. In a vertical testing machine the specimen is placed vertically. Machines of this kind are to be preferred for many considerations, whenever the specimens are not over 5 or 6 feet in length, so as to render this method of construction feasible. In such cases the machine may be built small enough, so as to be placed in an ordinary room, and may be conveniently manipulated from the floor; whereas, when the specimen exceeds the above length, a special building would be necessary; and when large and heavy specimens are to be tested it is exceedingly difficult to handle and place them in the machine with requisite care. Vertical machines may be built of any capacity that may be desired; but of course in making experiments on short specimens it is rarely desirable to carry this capacity above 300,000 or 400,000 lbs. For testing bridge columns, eye-bars, full-sized members of all kinds; angles, channels, etc., a horizontal machine, wherein the specimen may be placed parallel with the floor of the building, becomes almost a necessity. In a machine of this type the weighing apparatus is generally placed at one end of a long frame-work, designed to carry the reactionary stresses of the machine, while the mechanism for applying the stress, either a powerful hydraulic press or corresponding screws, is placed at the other. Over head, a traveling grip or crane may be conveniently arranged, and by this device specimens of any size or weight, may be readily and conveniently placed in the testing machine, supported

during the application of the stress, and the pieces removed as soon as rupture takes place.

The classification according to the method of applying stress into screw or hydraulic machines, simply indicates the method by means of which the force is applied to the specimen. In machines of capacity under 200,000 or 300,000 lbs., intended especially for experimental work, on the qualities of materials, screw apparatus is to be preferred for the following reasons:

First, the screw is absolutely positive. By arranging the driving mechanism, so as to turn the screws either rapidly or slowly, rapidity with which the stress applied to the specimen may be varied at pleasure. As soon as the screws stop turning, the whole apparatus remains absolutely rigid, and the stress may be continued on the specimen for an indefinite length of time. It will be readily seen that this is of especial advantage in the investigations calling the elastic limit and modulus of elasticity into question. For example, should the problem be presented to determine what load a specimen will, for a long time, sustain without detriment, the piece may be placed in the testing machine, the screws turned until the requisite load is indicated on the beam, and the poise left, with positive assurance that unless the machine is wilfully touched from the exterior there can be no yielding or springing from its position, and the load on the piece will not be in any way changed. Again, the method of applying stress by means of the screw is perfectly uniform and steady, provided that the gearing, driven by the engine or other motor, be run in a steady manner. The piece is, therefore, never subjected to any shocks or jerks or to any variation in the steady increase of the stress. The screw, however, is liable to some disadvantages, for example, the co-efficient of friction is so large that even in the best built testing machines, on this plan, an efficiency of more than ten per cent. is scarcely, if ever, realized. Again, in order to obtain the requisite speed for making tests rapidly, it is necessary to drive the auxiliary gear very fast, so that the screws may be rotated with sufficient speed.

For machines of more than 200,000 or 300,000 lbs. capacity, especially those designed for making experiments on full-sized members, where extreme accuracy of the testing machine is not so rigidly required, hydraulic power is far preferable. By means of a hydraulic press, and an accumulator, or three piston pumps, for continuously supplying fluid under a maximum pressure, stress up to any desired capacity may be readily and easily applied, as rapidly as may be wished. By means of an accumulator, the fluid forced in the jack may be maintained under a continuous and uniform pressure, so long as the accumulator will run. This piece of apparatus is, however, extremely bulky, if it be designed to supply fluid to a large or highly strained press. By means of a three-piston pump, the fluid may be supplied to the press with almost as steady a flow as that obtained from the accumulator, and of course by supplying a sufficient quantity of fluid to the pump that piece of apparatus may be made to work continuously so that a jack of any length of stroke may be operated without bulky machinery. Testing machines operated by hydraulic power have the advantage of extreme rapidity of action. For example, in making many thousands of tests for the East River bridge, conducted by the author, at the Cambria Iron Co.'s works, in Johnstown, it was customary to break a bar of steel an inch square and 1 foot long, stretching some 3 or 4 inches in from 6 to 10 minutes; a rapidity which it would be hardly possible to equal in a machine worked by screw power. There is, however, a serious disadvantage in the hydraulic testing machine, from the fact that the pressure is constantly varying, even with the best regulated pieces of apparatus. If it is wished for a moment to stop the action of the pump, either to take a reading on the stretch gauge, or for any other purpose, the beam drops almost immediately, showing that even the pistons of the best packed jacks do leak quite appreciably, and allow the stress to be relieved, unless some means is arranged for retaining a constant pressure on the fluid. Of course, in machines so constructed, it is impossible to ascertain the effect of a long continued stress, unless some means of clamping, or securing the piston rod can be arranged, to prevent this release of stress by leakage around the piston. The co-efficients of friction in the hy-

draulic machines are comparatively slight, so that in well-built machines of this class it is possible, as has been demonstrated by numerous experiments, to obtain an efficiency in the machine from 30 to 50 per cent. However, the constant leakage of fluids, around the joints or packings in various parts of the apparatus, gives rise to continued annoyance, and is apt to render the test room a dirty and disagreeable place.

The most important classification of testing machines is, that involving the apparatus for estimating the amount of stress to which the specimen has been subjected. The older machines of large capacity were generally arranged to have a fixed cross-head at one end of the frame-work, to which one end of the specimen was attached, while the hydraulic press for applying the stress was furnished with some form of gauge for estimating the amount of fluid pressure per square inch, to which the ram of the jack was subjected. By knowing the area of the piston, a simple example in multiplication gave the amount of stress to which the ram was subjected, and deducting therefrom an estimated quantity for friction, the result was assumed to be the stress to which the specimen was subjected. This, it has been recognized, was an exceedingly crude and only approximate method of obtaining that stress. The co-efficients of friction in the jack are exceedingly variable quantities. If the interior of the jack is smooth and well polished, if the packings have been in for a long time; and have become fitted to the cylinder, and are working at that part that has been most frequently used; if the jack is driven at a comparatively low rate of speed, and if the fluid is a good lubricator and free from grit and from any foreign substance, the co-efficient of friction may fall as low as $\frac{1}{2}$ or $\frac{1}{4}$ per cent. of the entire force exercised by the fluid. But if the interior be rough, the packings, new or working, in a part of the cylinder not often used, the co-efficients may rise to the extraordinary amount of 40 per cent. of the entire pressure placed on the ram. If this co-efficient of friction were a constant quantity and susceptible of being determined once for all, it would oppose no serious obstacle to the accuracy of the testing machine. But, unfortunately new packings and new supplies of fluid are necessary, and varying conditions of the same are essential, consequently this co-efficient is constantly changing, and may give rise to results on one day that are totally different from those on the succeeding one, so that no machine depending solely and entirely on the registration given by a gauge, of the hydraulic pressure of the fluid can be accepted as giving results that are constantly reliable and accurate. As an example of this may be mentioned the following experiment: A ram having an area of 90 square inches, was on one day worked at a pressure of 1,000 lbs. to the square inch, giving an effect on the scale placed at the other end of the testing machine of only 50,000 lbs., whereas, according to gauge pressure, there should have been a result of 90,000. On the day succeeding the experiment detailed above, and without there having been any change in any of the parts of the jack, a gauge pressure of 1,000 lbs. gave a scale reading of 85,000 lbs. What was the cause of difference has been an unsolved problem.

The next most common method of estimating stress is by means of the lever or combination of levers. In machines of this class the straining apparatus is so arranged as to be attached to one end of the specimen, while the other end is held in a cross-head directly attached to the levers forming the weighing apparatus. The following examples from some of the most prominent testing-machine makers in the country may be selected as exponents of this system of estimation:

Fig. 3 represents a 100,000 lbs. testing machine, built by J. L. Gill, of Pittsburgh. The machine consists of a heavy cast iron frame-work, supporting the driving mechanism, consisting of some gears and band wheel, seen at the right hand, which are arranged to drive a powerful screw directly at the left of the machine. This screw carries a cross-head to which one end of the specimen may be attached, while directly over it may be seen a second cross-head suspended from the large lever on the top of the machine. This lever connects, by a link at the left hand, with a secondary lever directly over the band wheel, which in turn is connected to the third lever, carrying two small poises and a weight counterpoise. The stress applied by the screws and the gears

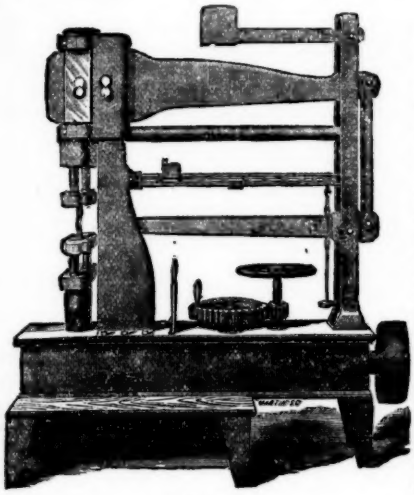


FIG. 3.

to the specimen is transmitted through it to the upper cross-head, then, acting on the poises and second levers, it causes the third, which is graduated as a beam, to rise. By sliding the poise in and out, and by adding weights to the weight counterpoise, the exact weight or stress applied may be readily ascertained.

In. Fig. 4 may be seen a very compact form of testing machine, built by Olsen & Co., of Philadelphia. Here one of the cross-heads of the machine, to which one end of the specimen is attached is supported on a large cast iron plate by means of four columns. This iron plate rests on the bearings of a large lever seen directly under the frame-work of the machine, which in turn transmits its stress to the beam at the right hand, where, in a similar manner to the previously described machine, the weight may be estimated by sliding the poises to and fro. Directly under the first mentioned cross-head may be seen a second cross-head, carried by means of four screws placed behind the columns supporting the upper head. By means of the crank and the gear at the left hand of the machine, this cross-head may be run up and down at pleasure, producing on the specimen the stress which is transmitted to the weighing apparatus.

Figure 5 gives a style of testing machine adopted by the Riehle Brothers, of Philadelphia. Here the frame-work of the machine carries a hydraulic ram supported at the bottom. On the frame-work stand the columns carrying the cross-head on the top, to which, as in the previous cases, one end of the specimen

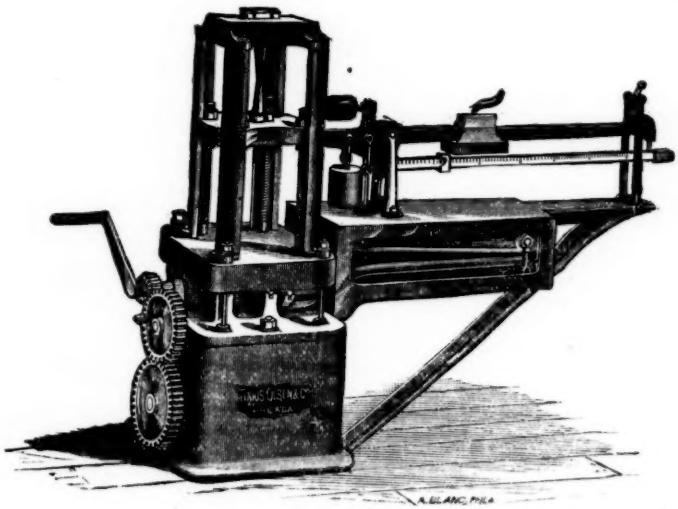


FIG. 4.

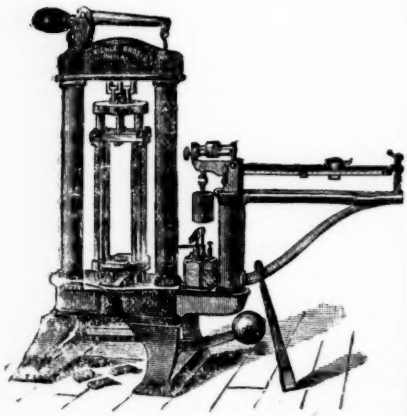


FIG. 5.

may be attached. Directly underneath the cross-head are four adjusting screws carrying a second cross-head. The object of these screws is simply to furnish a means of adjustability for raising the cross-head independent of the jack, so as to adapt the machine for a great variety of length of specimen. These screws are attached to a main lever concealed in the base of the machine which, as in the previous instance, also transmits its stress to the weighing beam at the right hand, where the stress, as previously, may be estimated by sliding the poises.

In Fig. 6 may be seen a typical illustration of the system employed by Messrs. Fairbanks & Co. A heavy frame-work of I-beams is arranged to carry the straining screws with their driving mechanism, and to support a scale system consisting of 7 levers. Four levers are arranged so as to support quite a large platform. From the ends of these 4 main levers, (technically so-called) the stress is conveyed to the central lever C, extending beneath the platform. From the end of this lever the stress passes to two multiplying levers, simply used for the purpose of reducing the amount of stress that is transmitted to the weighing beam. From the end of the last of these multiplying levers, the stress runs directly to the beam and may be estimated by the poises in the customary manner. On the top of the platform there stand two columns, supporting on their tops a large cross-head, to which one end of the specimen may be attached.

In the preceding machines it will be clearly noted that the straining mechanism has no connection at all with that part of the apparatus used for estimating the stress, except such as exists through the piece itself—the specimen forming the only connection between the weighing apparatus and that for producing the stress—consequently, it is obvious that only the stress which affects the piece can by any means (if the testing machine is properly built) produce an effect on the scale beam. Thus it will be seen that all co-efficients of friction, excepting such as may be involved in the levers and pivots themselves, is entirely eliminated from the testing machine, and only the stress used for rupturing the specimen is ever indicated.

The remaining class of testing machines, viz., those that estimate the force applied to the specimen by a hydraulic scale has but a single representative—that of the famous Emery testing machine.

Fig. 7 gives representation of the machine now at the Watertown Arsenal. Here it may be seen that at one end there is an exceedingly large and heavy-weighing mechanism, from whence proceed two screws extending nearly the whole length of the room and carrying on them the hydraulic straining jack with the jaws for holding the specimen. Essentially, the weighing mechanism consists of a large receptacle called a hydraulic support, covered with two thin flexible diaphragms. This receptacle is filled with a fluid made of a mixture of alcohol and glycerine, and connected to a similar receptacle of much smaller dimensions, placed under the weighing beam. According to the elementary principle of hydrostatics, the total pressure on a surface is proportional to the pressure per square inch and the area of the surface. If, consequently, it be supposed that in the main hydraulic support there is an area of one thousand square inches, and if in a similar miniature hydraulic support under the beam end, there is an area of only 1 square inch, a thousand pounds on the main support will only produce 1 lb. under the beam. It will thus be seen that by varying the relative

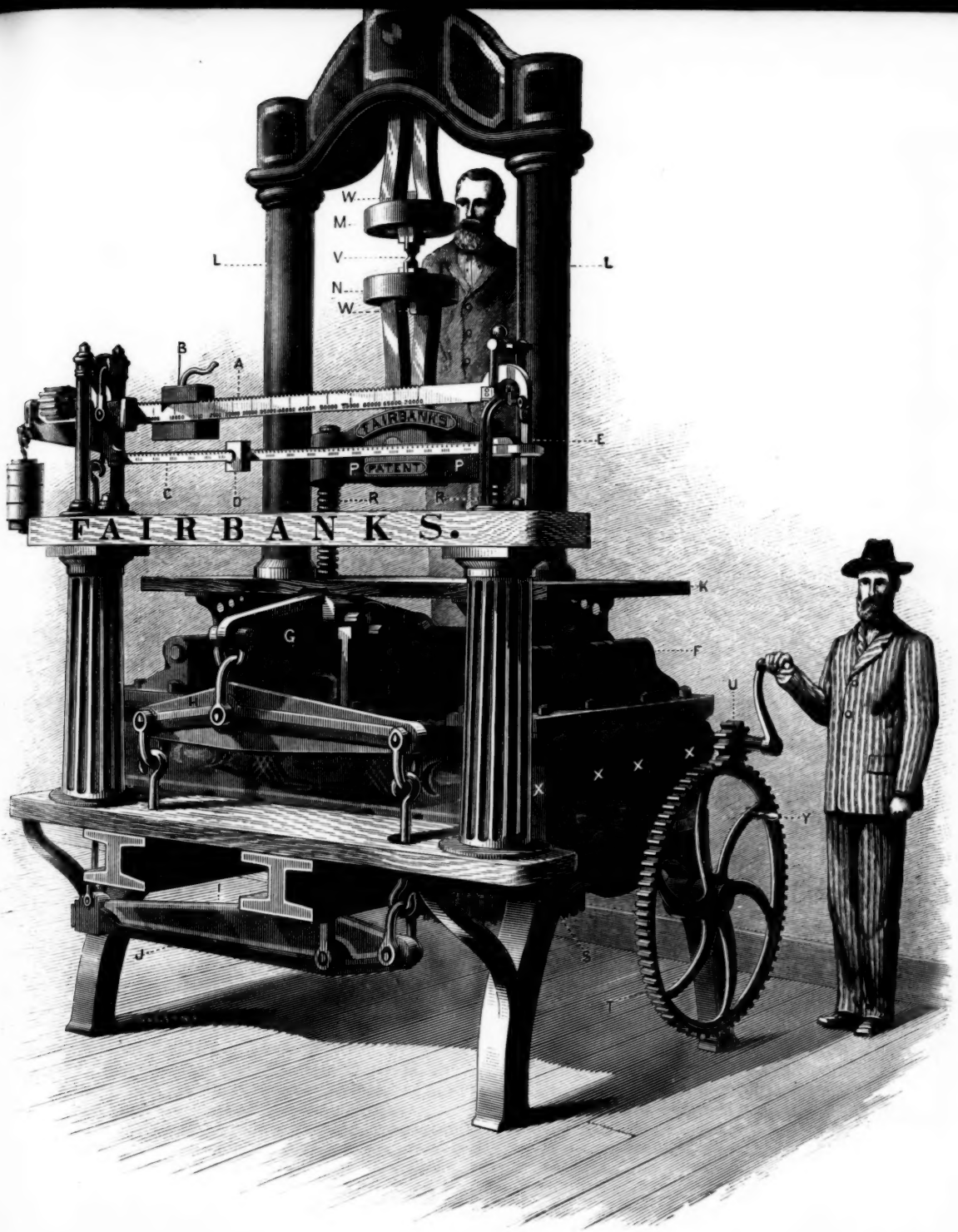


Fig. 6.

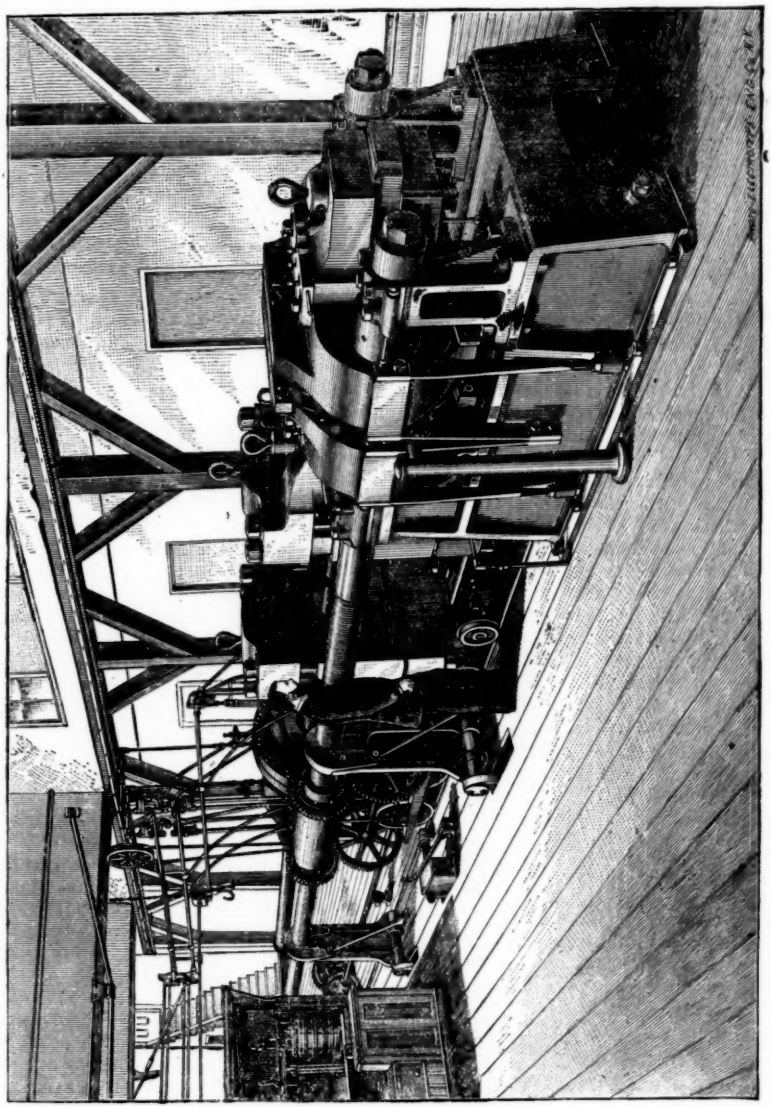


FIG. 7.

sizes of the two hydraulic supports any desired multiplication may be obtained under the beam. The weighing beam, instead of being supported on the customary knife edges, is hung by one or more metal strips. This arrangement will be seen to constitute an absolutely friction-less piece of mechanism, for the pressure coming on the large diaphgram is transmitted by the fluid column to the weighing beam, and simply operates to slightly bend the metal constituting the diaphragm and the suspending strips of the scale beam. Thus, in the true sense of

the word, the weighing is affected by no friction whatsoever, the only opposing force being the molecular resistance to flexure of the diaphragms and the suspending strips. The great merit of this ingenious device is its extreme sensibility, the enormous machine at the Watertown Arsenal being perfectly sensitive to the application of a few ounces of stress between the cross-heads.

ON SECONDARY BATTERIES AND THE ELECTRICAL STORAGE OF ENERGY.

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The ordinary methods of storing energy at present in use may be divided roughly into mechanical, chemical and electrical. As an example of the first kind, we may take the raising of weights, which is accomplished on a large scale hydraulically, and is applied to the working of cranes and dock-gates; another example is the coiling of springs, as in clocks and musical boxes. An example of a chemical store of energy is gunpowder; another is zinc or other smelted metal; and a third example is vegetation, whence arises all the energy of coal and gas engines. The charging of a Leyden jar, and the decomposition of dilute acid, furnish examples of electrical storage, and the explosive gases derived from the last-mentioned operation could doubtless be used to drive some form of gas-engine.

All these methods of storing energy involve some loss, and many of them are accompanied with a great deal of loss. The advantage of being able to store energy frequently outweighs the disadvantage of a considerable amount of loss of energy in the process; and there are three main cases in which this occurs.

One is when regularity and continuity of supply of energy is needed, and when the source of energy to be utilised is irregular and fitful. For instance, it would be convenient to be able to store wind or wave power, and utilise it in equable and dependable manner.

Another case is when occasional power is needed at times when the source of energy is not acting, or when a very great power is required for a short time, whereas the source of energy at your command has a steady but insufficient power. This would be exemplified in the utilisation of a small engine for lighting purposes.

The third case is when the power at your disposal is so great and so unutilised that the saving of even a fraction of it is a clear gain, no matter what becomes of the rest. This case frequently occurs when natural water-power is utilized. Processes of vegetation also utilize but a meagre proportion of the solar radiation, but the small proportion saved and stored up in the coal fields represents so much clear gain.

The mode of electric storage which has recently excited so much interest involves the use of a secondary battery, which may be defined as an instrument for receiving the energy of an electric current, and for giving it out again in the same form. This is a case of storage without transmutation, such as is also accomplished in the simple operation of winding a clock, where mechanical energy is taken in, to be subsequently given out in the same form. But in the electro-chemical decomposition of acid, the energy of an electric current is stored in such a way as to give sound, and heat, and light, and motion, but not an electric current under ordinary circumstances. There is one arrangement for making the evolved gases directly furnish an electric current, and that is a true secondary battery, viz., Grove's gas battery, or a voltameter.

A primary battery means one in which the plates have been made, or prepared and rendered active, by chemical means; a secondary battery means one in which the activity of the plates depend upon electrical energy having been first expended upon them. Any two conducting bodies, immersed in a conducting liquid

able to chemically attack one of them more than the other, constitutes a battery; and the difference in the attackability of the two bodies or "plates" may be caused either by making them originally of different materials, or else by modifying their surfaces in some way, permitting the main bulk of them to remain identical.

Most run-down batteries can be re-excited by forcing a reverse current through them, and they then become true secondary batteries.

The polarization of voltameter plates was studied by Gautherot, Ritter and Grove, in the earlier years of the century; and more recently and exhaustively by Planté, who found that plates of lead acted extremely well, and retained their energy for a long time.

The gas battery consists of two similar platinum plates, one soaked, or coated, or alloyed with hydrogen, the other with oxygen. But the quantity of oxygen which platinum can store up is very minute; the advantage of using a metal actually oxidizable is manifest. Platinum, and still more palladium, have considerable holding power for hydrogen, but by the use of a salt or an oxide as a recipient of the hydrogen, a far greater amount of reversible action can be stored up than by attempting to store up the hydrogen itself by any power of occlusion.

A Planté cell, from the beginning, furnishes an oxidizable plate on the positive side, but its negative plate has at first no practical occluding power for hydrogen.

Reversing a once charged Plante cell, however, we get metallic lead for the oxidizable plate, and an already oxidized, and therefore reducible, plate to receive the hydrogen; the oxide gradually being reduced to metallic lead, of a porous or spongy consistency. By many reversals of this kind, Planté "formed" his cells, until the lead plates were deeply acted on, and a considerable storage capacity obtained.

The modification of Faure consisted in providing both an oxidizable and a reducible porous material from the very first, by coating the plates with minium or litharge. The plates could then be peroxidized, and reduced respectively to a considerable depth, without any tedious process of "formation." A considerable storage capacity was thus obtained, but difficulties presented themselves connected with the adhesion of the coatings to the plates, and these were not got over until perforated plates were employed to hold the applied substances. Perforated plates, by rendering unnecessary any jacket of cloth or felt to keep the coating on, and by thus permitting a free circulation of the liquid, no doubt are a considerable improvement; and cells, as now made, often work extremely well; at the same time, a layer of uniform thickness spread all over the plate, and not locally distributed in a non-homogeneous manner, would seem theoretically to be preferable, if the practical difficulties connected with such a uniformly spread layer had not seemed too great. Plates made in the old manner did not safely bear transport and rough usage, and a film of insulating sulphate of lead was apt sometimes to creep between the metallic lead and the peroxide coating, and to destroy all electrical connection between them over large patches.

The process of manufacture, as now carried on, is here illustrated by a number of specimens of plates in different stages, kindly lent for the purpose by the Electrical Power Company, of Millwall.

The first operation is the casting of the lead grid, the holes in the plate being from $\frac{1}{4}$ in. to $\frac{3}{8}$ in. square. The positive grid is made a little thicker than that which is to be the negative, for reasons which will shortly appear. The thickness of the grids at present most employed are, positive, $\frac{1}{8}$ in. to $\frac{1}{4}$ in.; negative, $\frac{1}{12}$ in. to $\frac{1}{8}$ in., and they are made in two sizes, one $7\frac{1}{4}$ in. by $5\frac{1}{4}$ in., the other 9 in. by 10 in.; the former being used in the small or "half horse-power" boxes, the larger plate being used in all other sizes, it being found more convenient to vary the number of plates in a cell than to have in stock a number of different sized plates. The weights of the formed plates are, small size negative, 15 oz.; small size positive, 21 oz.; large size negative, 50 oz.; large size positive, 70 oz., about.

The next operation is the filling of the grids with a paste of oxide of lead mixed with dilute sulphuric acid. The negative plates are filled with litharge, PbO, the positive with minium, Pb_3O_4, since there is an evident economy in having to reduce only the lower oxide, and to peroxidize only the higher one.

The plates are then taken to be "form-

ed" by the electric current, for which purpose they are packed alternately in square stoneware jars filled up with dilute sulphuric acid, and a current of about two or three amperes per pair of plates supplied. At the first instant of immersion in acid, a darkening of the minium occurred, owing to a partial formation of peroxide in it by the action of the acid; and this darkening continues, under the influence of the nascent oxygen liberated by the current, until the whole of the composition is converted into peroxide. The litharge is, on the other hand, gradually reduced to metallic lead by the action of the nascent hydrogen produced by the current, the reduced lead having a slightly porous or spongy consistency.

No gas is evolved from the cells for some time; it is all consumed in effecting these chemical changes. A few minute oxygen bubbles do, however, make their appearance on the positive plate, much of it being in the form of ozone. Ultimately, as the surface of the positive plate gets peroxidised over, a good deal of oxygen escapes, and it is only after a long-continued current that the portions of minium in the interior get acted upon. In time all the minium is attacked, however deeply buried; the reason being that less E.M.F. is needed to send the current to an oxidizable substance than to saturated peroxide. The reduction of the litharge is a somewhat slower process, but the plates being thinner, it is accomplished in approximately the same time, which may be taken as between three days and a week. When the process is approaching completion, torrents of both kinds of gas rise from their respective plates. The plates are then removed from the forming pot, rinsed plentifully with water to remove the excess of acid, and then stacked till wanted. Unless the rinsing operation is carried out, the spongy lead plates are found to heat considerably, by reason of the acid in their pores combining with the lead.

In order to keep the plates from touching each other when subsequently used in battery cells, various devices have been tried. The one now most in favor is to punch out the composition from a tolerable number of the holes of each negative plate, and to insert instead a block of vulcanised India-rubber in each. These plugs project nearly a quarter of an inch on either side the face of the plate, and effectually prevent the adjacent positive from coming into contact with it.

Another matter which has given some trouble to determine is the best method of connecting the plates together. All the positive and all the negative plates in a cell have to be electrically connected in the most complete and permanent manner. Clips and clamps of various kinds have been tried; but, to guard against all possibility of corroded surfaces and imperfect connection, it is now thought wise always to use solder, whether in combination with screw clamps, or otherwise, and to protect the joint from the action of acid spray by a varnish, or by a mass of paraffin. It is curious to notice how persistently the junction of the + plates tend to corrode, showing how the oxidizing action creeps up liquid films on the lugs.

When the plates are to be used, they are packed alternately in glass or ebonite boxes, the India-rubber plugs keeping them apart, and wooden supports keeping them off the bottom of the cell. Their lugs are connected together, and the box is filled with dilute sulphuric acid, say, of specific gravity 1.100 or 1.150. A charging current is then supplied, so as to freshen up the plates, and they are then discharged, and charged up again once or twice, to bring them into good condition. It is not a good plan to discharge them too completely, nor to let them remain idle for very long together; neither should they be charged or discharged too rapidly, so as to heat them above a gentle warmth. The more regularly they are worked—as for instance, when they receive a charge every day and discharge every evening—the better they behave. Regularly and carefully worked cells will keep in good order for a very long time, but if they remain idle for weeks together, some of the plates are apt to warp, and otherwise give trouble and require attention. They must not be overcharged very excessively either. When full, they indicate the fact by evolving a good deal of gas, and it is best not to continue the charging power much longer when this stage is fully reached, partly because nearly all the charge then put in is wasted, but principally because a little of the oxygen is absorbed by the lead of the grid, and it therefore gets slowly peroxidized,

so that, after a sufficient lapse of time, it may get actually eaten through, and the plate become friable. When a plate has got into this state, the only plan seems to be to melt it down with a flux, and recover the lead. But the complete peroxidation of a plate by overcharging is a work of time, by far the greater quantity of gas being harmlessly, though wastefully, set free; and it is not necessary to be alarmed at a slight evolution of gas which usually goes on from the positive plate when being charged, more especially as the cell is getting full. Alarm at the appearance of bubbles may lead to the cell never being properly filled up, in which case the battery will fail to work satisfactorily.

The number of plates which are packed into each box depends entirely upon the size of the box, and it is in this way that the different sized cells are constructed, not by varying the size of the plates; except that the smallest cells are made with half-sized plates.

Taking the full-sized plates, we may reckon that each pair is able easily to receive and give out electricity at the rate of four amperes per day. This, of course, reckons both faces of each plate as active. This process may be continued for say 8 hours on the average, consequently the storage capacity of a cell is 32 ampere hours per pair of plates. The E. M. F. of discharge is as nearly as possible 2 volts; consequently the power of a cell is 8 watts per pair of plates; and the total available energy per pair of plates, when full, is 8 watts for 8 hours, or 170,000 foot-pounds; which may be also, though clumsily, expressed as 1-12th of a horse-power-hour per pair of plates. Consequently, a box which is to give 1 horse-power-hour should contain 12 pair of plates; a 2 horse-power box 24 pair, and so on. But each box will not really give a horse-power for an hour, it will rather give an eighth of an horse-power for 8 hours. If a horse-power is wanted, all you need do is to take 8 cells, and then they will give you a horse-power and will last 8 hours; 16 cells will give 2 horse-power for the same time, and so on. By drawing off the power more slowly, the discharge will last longer, and it is more economical thus to work, except as regards first cost of batteries. This latter item it is which usually causes them to be called upon to exert power up to their maximum reasonable capacity. It is true that a cell will give a much higher power for a short time if called upon—thus a single cell might possibly be made to give a horse-power, but then it would not last out its proportionate time. I have found some cells, which I discharged at a very moderate rate, continue discharging an extraordinary time, and give out sometimes even more than was put in, which, of course, means that the effect of previous charges was being drawn upon.

When a cell which is being rapidly discharged has apparently run completely down, a few minutes rest will suffice to recover it a little; and after an hour or two's rest a cell will commonly have recovered sufficiently to enable it to discharge with something of its old vigour for a period comparable with that of the rest. This recuperative power of the cells is in some respects advantageous, but, on the whole, it would be better if they gave out the whole of their energy continuously without needing intervals for refreshment. The more slowly a cell is discharged, the less noticeable is this temporary falling off and subsequent recovery.

When a cell has run down, it is interesting to examine which of its three constituents is the one that has failed; the positive plate, the negative plate, or the acid. Sometimes, especially in cells too crowded with plates, there is not enough acid present; the free acid is absorbed by the lead, and only water is left. The specific gravity of the acid may, however, be easily watched by floating a little hydrometer in the cell. More frequently the negative plate is the first to fall off; as may be shown by removing it and inserting a fresh one, when the cell will go on again. The cause of this, and of several other peculiarities observed in batteries, may be readily made out by experiments on a small scale, on two little plain lead strips, held in a convenient clamp, and immersed in dilute acid.

To a pair of such strips of sheet lead supply a current, from a couple of Groves, say, for half an hour or so. The positive plate is seen to become coated with a pure layer which gradually darkens to black. The other plate seems unchanged. Gas

is evoked from both plates. On now connecting them with an electric bell, it will ring for a minute or more. Now recharge, but this time, instead of discharging the cell, remove the negative plate, and insert instead a bit of clean lead, the same in appearance as the one against which hydrogen has been so long liberated, and then discharge the cell; it will only ring the bell for about two seconds. Depress the plain lead plate further into the acid, so as to expose a fresh surface, and the bell will give a few more strokes. Remove this plate altogether, and re-insert the hydrogenized plate which had been used for charging the cell, and the bell rings vigorously for as long a time as it did in the first experiment, showing that the peroxide plate is perfectly vigorous, and showing also that the nascent hydrogen has modified the lead in some way or other.

The natural supposition is that the lead has occluded hydrogen, but Dr. Frankland has shown that, if such hydrogenized lead be melted down, it gives off no hydrogen worth mentioning. It is not easy to see wherein the change consists. Under the microscope, the hydrogenized plate looks a little darker than the plain plate, but no real porosity is noticeable. The fact, however, is certain, that it behaves in a very different manner to plain lead, and that it retains this singular power long after it has been removed from the liquid and rinsed.

To avoid misunderstanding, it might have been well to state earlier that I use the term "positive plate" to signify that at which the charging current enters, and from which the discharge current leaves. It is that one whose terminal is colored red in the cells of commerce. It is that which must be joined to the + pole of a magneto-machine, whether the latter is to act as a generator or a motor. I make no distinction between positive plate and positive pole, because all such distinctions end in confusion.

The plain lead plate which refused to give a current when opposed to peroxide, did not fail because of its high position in the electro-chemical series, as may be proved by opposing it to the hydrogenized plate, when it is equally inert; it fails simply and solely on account of a film of some non-conducting substance which spreads over its surface the instant it is used as one electrode of a battery, the substance appearing to be sulphate of lead. The internal resistance of the cell thus becomes enormous, and the current ceases. On standing idle for a time, the film has a chance of clearing off—though from a plain lead plate I do not notice much tendency to clear—or it may be wiped off with a cloth at once; but a fresh film speedily forms. If amalgamated lead be used, the white scum can be seen, and it sometimes drops off in a filmy sheet. One remarkable way of instantly clearing off the scum is to touch the lead under the liquid with a scrap of amalgamated zinc. The effect is magical: the scum vanishes, and the bell again rings for several seconds after the zinc has been removed. So long as the zinc is present the current will continue, until indeed the positive plate becomes exhausted.

To examine whether the positive plate also exhibits the temporary falling-off, and to eliminate the concurrent action of the negative, the best plan is to use a piece of amalgamated zinc for the latter; for since sulphate of zinc is soluble, it remains perfectly clear, and all variation is traceable at once to the behavior of the positive. Under these circumstances, the cell, if rapidly discharged, does show a slight recuperative power, recovering somewhat on standing. So far as I can make out, this appears to be mainly due to a temporary exhaustion of the acid close to the surface, and in the pores of the peroxide, so that the power falls until, by rapid diffusion, the absolved acid has been replenished. The insulating scum of sulphate on the negative plate is, however, by far the most powerful cause of the ordinary running down of a cell.

When we come to try the hydrogenized plate employed in charging the cell instead of the plain plate, we find that it also ceases to act before the positive is exhausted, though it is able to go on for a considerable time; it ultimately acquires a scum. On standing, the scum clears off partially, and the cell recovers. A piece of zinc made to touch the plate under the liquid clears it, and in a short time restores it to its original power, having precisely the same effect as a reapplication of the charging current. The fact is not surprising, since the zinc naturally causes nascent hydrogen to be

liberated on the plate, just the same as a current would; but the quick and prompt action of the merest scrap of zinc is somewhat striking, and some practical application of the fact would seem to be possible.

Instead of a plain lead plate, lead sponge may be employed for the negative plate, such as is made by Watt, of Liverpool, by blowing high pressure steam through a jet of molten lead, or such as Mr. Desmond FitzGerald has brought me here, made in some other way. The enormous surface of this spongy lead enables it to go on acting for a much longer time than plain lead, without being stopped appreciably by a scum. This effect is due to the extent of surface, and it appears to be owing to a sort of potential hydrogenation caused by the ability of acid to attack spongy lead, or any metal in a fine state of division, more easily than when massive; and the sulphate of lead formed, though it certainly clogs up the pores, is not readily able to form a continuous protecting film.

A thin coat of peroxide formed on a lead plate rapidly becomes lighter in color when the current is stopped, being evidently reduced to a lower oxide by local action with the lead underneath. When the coat is very thin, the changes go on so rapidly that, by quickly pressing the charging battery key down and up, flashes of peroxide appear and disappear on the surface. It is by this local action effect, no doubt, that peroxidation eats deeper and deeper into metallic lead, for otherwise, as soon as a complete layer of peroxide was once formed, all subsequent gas would be given off from its surface (it being a conductor), and the lead beneath would be protected.

The local action proclivity between lead and peroxide has two unpleasant consequences, however; one is that the peroxide gradually disappears on long standing, and the cell thus loses its charge; the other is that the metallic lead of the plate gets gradually eaten into, and corroded away. These two results are modified in practice by the fact that, whereas local action destroys the peroxidation of thin films in an instant, in the deeper layers of thick films it goes on very slowly, partly because fresh acid cannot so easily penetrate, but principally because the sulphate and lower oxides formed, being non-conductors, exert a clogging and protective action.

Instead of using a lead negative plate, a copper-plate may be used, and will give a strong and lasting current. It lasts, of course, because copper sulphate is soluble, and no insulating scum is formed; but that this Sutton cell has a fairly high E. M. F., is a striking proof of the value of peroxide of lead as the electro-negative element of a battery.

Even platinum gives a current when opposed to peroxide of lead, though of course it is a feeble one. But if zinc is used, we have the strongest practicable battery I know, each cell having an E. M. F., of 2½ volts.

I must now say a few words about the methods and instruments for practically measuring the various quantities involved in the storage of energy by secondary batteries. We need current or amperemeters, potential or volt-meters, quantity or coulomb-meters, and energy or erg-meters. The well-known dead-beat ammeters and volt-meters of Professors Ayrton and Perry are exceedingly convenient instruments, especially the former; the only objection to them is the frequent calibration they require, because their permanent magnet changes in strength. When portability is not essential, I find a high-resistance galvanometer, of the form known on the continent as the "Wiedemann," a most accurate and convenient volt-meter. The needle has a thick copper damper, which makes it almost dead-beat, and a resistance of 30,000 ohms or so has to be added to the circuit, in order to bring the deflection given by one cell within moderate range. It thus taps off an absolutely infinitesimal fraction of the current.

The deflections are immediately interpreted by observing, at the same time, the reading produced by a couple of common Daniell cells in series, each of these being assumed to be 1.12 volts. There is a trifle of uncertainty about the absolute E. M. F. of even a freshly set up Daniell, and this uncertainty will continue until an authoritative standard of E. M. F. is issued by the British Association Standards Committee; but variations in E. M. F., one-thousandth part of the whole, can be readily detected; and a sensitive volt-meter is somewhat important, if fouling cells are to be detected in their

early stages, and before the cell is seriously injured.

As a meter for the quantity of electricity supplied or withdrawn to the cells, I use a large copper drum, suspended in sulphate of copper on the end of a balance arm, in such a way that it can be weighed at any moment without withdrawing it from the solution. Contact is easily made with it by means of mercury, and it is surrounded with a larger cylinder of copper, to act as the other pole. Allowing for the buoyancy of the solution in which it is weighed, every gramme increase or diminution of the drum means 1.04 ampere-hours put into or taken out of the cells. Frequent weighings of the drum enable one to get a very good idea of the cells as regards charge, and they keep an accurate record of the loss, or discrepancy between the charge and discharge quantity.

A copper-drum coulomb-meter may also be made floating, so as to indicate by its height the quantity of electricity which has passed through. If such a vertical-moving quantity indicator carries a sheet of sensitised paper, on which the horizontal spot of light from a volt-meter records a trace, the arrangement will give an indicator diagram of the charge and discharge precisely analogous to the indicator diagram for the steam engine, except that the area enclosed by the curve represents the loss of work instead of the balance of useful work. The useful work is indicated by the area between the discharge curve and the two straight lines of reference.

Many plans can be devised for combining the two traces, the quantity and the E. M. F., at right angles to one another, and the continued use of some such instrument would, I believe, assist in detecting the peculiarity of cells, and in improving their manufacture and system of treatment, almost to the same extent as the indicator throws light on the peculiarties of different steam engines. But even without such an instrument for automatically constructing the diagrams, a curve plotted in this way (with quantity for abscissa and E. M. F. for ordinate) is the readiest mode of exhibiting the results of measurement taken with ordinary meters, and making the value of the cell manifest at a glance.

A highly ingenious quantity-meter has been devised by Dr. Hopkinson, and since it indicates precisely like a gas-meter, it is easily intelligible, and may come into general use. But at the present, I believe, it only works one way, hence it is not so directly applicable to secondary batteries; it would add up both charge and discharge in one continuous record, drawing no distinction between them. The simplest power or watt-meter is the Siemens dynamometer, arranged with one series and one shunt coil; and it indicates at any instant the power which is being supplied or withdrawn from the cells.

An erg-meter, or energy integrator, can be made in various forms. The one I have here to show is Ayrton and Perry's modification of an eight-day clock. They substitute a fine coil of wire for the bob of the pendulum, and surround this with another larger and thick coil. A current sent in the same direction round two nearly concentric coils, causes the smaller one to move to the centre of the large one. Thus, if the pendulum is oscillating, and the clock keeping good time, a current sent round the two coils will cause the clock either to gain or lose, according as the currents are opposed or agree in direction. If the two coils are both in series in the circuit, the gain or loss of the clock will indicate the coulombs passed through. But if the small fine coil is a shunt, while the other is a series, the force acting on the bob will be a function of the power, and the gain or loss of the clock will indicate the energy in some more or less arbitrary way, which can be made simple proportion by empirically adjusting the relations among the parts.

To determine the internal resistance of a battery cell, or of a whole battery, the plan is to measure its electromotive force when driving two different strengths of current, as different as possible and both known. One strength of current is most conveniently zero; the other must be measured, say, by an am-meter in the circuit. A volt-meter, such as the high resistance Wiedemann, is connected to the terminals of the battery and read. Its indication will be a scrap higher if the current was being supplied by the cells, but lower if the current was being supplied to them. The difference of the indication of the volt-meter, when a current is passing and when no current is passing, is proportioned to the resistance

of the battery, and for a secondary battery cell in good order, the difference is surprisingly small, so much so that an ordinary volt-meter will often fail to detect it. To get the resistance of the battery in ohms, you must divide the difference in the readings of the volt-meter (interpreted into volts) by the strength of the current passing in the main circuit, reckoned in amperes. A moment's interruption is sufficient with a good dead-beat volt-meter, and the current may be immediately switched on again before the disturbance in the circuit has had time to show itself, except indeed where the current is being used for lighting purposes, in which case no tricks may be played unless there are several batteries in multiple arc, and one of them can be disconnected without detriment. This, indeed, is a good plan of connecting batteries, as it allows measurements, examinations and repairs to go on while the current is being used. The resistance of one of the small sized (half horse power hour) cells with nine pair of plates averages .002 ohm. Large cells have a still lower resistance, of course, and it is to this extremely low internal resistance that the secondary battery owes much of its great practical value. The current from an ordinary Grove's battery is by no means proportionate to the external resistance of the circuit unless this is high; in other words, the driving power of a cell, or the difference of potential between its terminals, falls off greatly when strong currents are demanded. But a secondary battery, from its enormous area of surface, not only gives a higher E. M. F. than a Grove ever does, but it keeps this two-volts difference of potential between its terminals for pretty nearly any reasonable strength of current. Hence the current does within wide limits vary with the external resistance, and the current which such a cell will drive through a piece of thick copper wire is simply tremendous.

I must now say a few words about the uses to which secondary batteries can be applied; and, first of all, for general laboratory purposes they are invaluable. A small gas engine and shunt dynamo are necessary to charge them up occasionally, but they then retain their charge for days or weeks, according to the demand made upon them, and they are ready at any moment to produce a current, strong or weak, according to wish. They thus save a deal of trouble in setting up Grove batteries, and, moreover, the current they give is much steadier, for unless they are run too near their falling-off point, their E. M. F. continues very constant, and no compunction is felt at leaving them connected up to the circuit for a long time together—for such a time as would make Grove cells heat violently and boil over. When they are run down, a few days' charging replenishes them again.

As regards durability, the cells repay cleanlness and attention. I mount them in front of a window, on a slab of thick glass raised above the bench; one can then see all over and through them, the light being reflected from below the glass slab by an inclined bit of looking-glass which can be moved about. It ought also to be possible to get on both sides of the bench, so as to be able to detect and remove any attempt at contact between the plates of a cell.

If a lump of composition tumble out and bridges across from one plate to the next, it can be removed with a paper-knife. Plenty of light, easy access, and clear vision through between the plates of each cell are highly desirable. The glass cells and the slab should also be kept clean, and free from acid splashes and spray, for all this moisture promotes leakage, and causes the cells to lose their charge more rapidly than they would by ordinary and unavoidable local action.

When cells are stowed away in dirty dark cellars, on wooden shelves, like wine bottles, the leakage must often be considerable, and the cells may get into any state of dilapidation without detection or attempt at remedy. A light attic or glass house, with crockery slabs at a convenient height, is a much more suitable place for secondary batteries than a wine cellar.

It is important to be able to detect an incipient flaw in a cell before it has had time to damage the cell perceptibly. A sensitive volt-meter may do this, but an ordinary one will not show any difference for a long time, and if the cell is in series with others, as soon as it begins to run down in E. M. F., the others rush it down to and past zero, and begin to charge it up wrong way. This reversal is very bad for a cell, besides its very weakening effect upon the main current.

It is very important to keep all the cells of a series as exactly alike as possible. A bad short circuit often causes a hissing noise which can be heard with a stethoscope applied to the cell, even when it is not loud enough to call your attention, as it sometimes is. But the most delicate detector of an incipient short circuit is the thermometer. All the cells heat by their proper current, but any abnormal action going on in a cell, whether it be short current or local action, or any other dissipation of energy, must result in heat, and the thermometer is the natural detector of this. Any cell warmer than the others should be examined, and set right or else replaced.

The lugs and contacts should be wiped clean occasionally from the acid spray which collects on them, and they may also be recoated with paraffin and shellac. This acid spray is thrown up by the bubbles of gas which rise when the cell is getting full. The hydrogen bubbles throw up largish globules, which bound two or three inches into the air, and fall all over the slab and terminals; but when hydrogen comes off freely, the cell is commonly quite full. The oxygen which comes off slightly during the greater part of the charging throws up much more minute globules, which float about as an acid spray, very irritating to the nostrils and very provoeative of leakage. The spray may be very greatly diminished by a layer of oil spread over the surface, in which the bubbles collect and break in a different way, and it also checks evaporation; but the oil introduces difficulties of its own when the plates have to be lifted out of the cell, though indeed it may be first flooded off. The contacts of the positive plates are very apt to be corroded by the spray and creeping film, together with the per-oxidizing action of the current crawling up wet lugs, and they require occasional attention. Unless absolute uniformity of contact is secured, some of the plates will take nearly all the current, and get peroxidized to pieces, while the others are nearly idle.

I use pans of very dilute ammonia, or perforated boxes of carbonate, to neutralize the acid spray. The atmosphere is then much improved, and instead of acid films, white crusts of ammonia sulphate collect, which are less harmful and may be cleared off. Another precaution is always to keep the cells filled with acid above the level of the plates, so that they are completely immersed, else the part above the liquid will be exposed to the influence of non-concentrated acid, from the evaporation of the water out of the acid film covering it. It is important to remember that all these exposed layers of acid are likely to become pretty concentrated, and hence, that they may be expected to corrode more rapidly than the mass of the solution.

Another advisable precaution is to stir or agitate the liquid occasionally, more particularly during charging. The bubbles of gas, fortunately, do this to some extent, if the cells are well filled up electrically; but if no such agitation is excited, the acid at the bottom of the cell is apt to become very much more concentrated than that at the top. Not that gravity is able to pull down strong acid out of once thoroughly mixed dilute acid. Gravity cannot undo diffusion like this. But it is the acid freshly formed during re-charging, by decomposition of the sulphate of lead on both plates—it is this acid which falls to the bottom and accumulates there, frequently corroding the bottoms of the plates. One great advantage of the present cells over the old flannel-swathed Faure, is that they permit free circulation of the liquid. To make the cells answer various laboratory requirements, I have a sort of resistance-box, made with uncovered spirals of German silver wire, in a stoneware water trough, so that the wires do not melt, and I can then throw in any resistance from a quarter ohm upwards, otherwise the current would be frequently too strong. For working a Serrin lamp no extra resistance is necessary, and from eighteen to twenty-two cells work it admirably. Even when one drives the lamp direct from the dynamo, the battery is still connected as a shunt circuit, to act as a regulator and steady the current. Moreover, it has the great advantage of preventing the engine from racing away when one puts out the lamp. A well-governed engine, of course, ought not to do this in any case, but I have not found a gas-engine behave well, when work is thrown on and off it in such an irregular and fitful manner as a lamp is commonly used in a lecture. It is much better for it to be pumping into the cells

whenever the lamp is put out. Moreover, one can get a far more powerful current through the lamp in this way than from a small dynamo direct. Another advantage of having the battery connected is, that it will not permit the locking of the carbons, and thus the short circuiting of the field magnets of the shunt dynamo, which is a not infrequent occurrence when a shunt dynamo alone is used to feed an arc lamp.

If, however, only a regulator is wanted, and not a store of energy, a set of zinc and lead plates can be easily arranged, which will do all that is wanted in the way of regulation; and as the lead gets gradually more and more "formed," the battery will gradually acquire more and more actual storage capacity. The cells were very useful once when the gas-engine was being repaired. I took the dynamo over to a neighbouring steam-engine, and laid its comparatively full current with spans of common wire to the battery. From the battery thus charged, the powerful occasional currents needed for some magnetic researches then going on were readily obtained. To bring such currents from the distant dynamo would have necessitated the use of very thick and expensive cable, and would in fact have seriously damaged its armature.

The battery thus acted as a transmuter of weak currents into strong ones, and this illustrates on a small scale one of its very important future functions, viz., a receiver of energy transmitted from a distance, as Sir Wm. Thomson has pointed out. Currents generated at a distant station, to be conveyed to a distance, ought not to be strong; they ought to be feeble currents at a high potential. They can be received by a great number of batteries in series, and the batteries so charged can be used for any local purposes, and made to give any reasonable desired current by altering their connections. Moreover, they can be coupled up to a number of independent circuits, so that irregularities in one shall not interfere with the others. The great advantage of a set of batteries as a receiver, over a set of dynamos in series, is that the latter would need such a high internal resistance to give the necessary back E. M. F., whereas a battery, even of small cells, can give 1,000 volts E.M.F., with an internal resistance of only 2 ohms. Hence the waste in heating the receiver is next to nothing. There is another kind of waste, no doubt, for the work got out of a battery can never be anything like that put in. The E.M.F. of charge is about 2.3 or even 2.4 volts, while that of discharge is scarcely above 2; besides this, there is a slight loss or discrepancy between the quantity supplied and that afterwards withdrawn. Notwithstanding this waste, they must come into use when made in a satisfactory and quite dependable manner, for their great convenience and steadiness. And when the power at the distant station is water or wind-power, as I doubt not it often will be, waste becomes of secondary importance, and a convenient method of storing is the first consideration.

So plainly is high E.M.F. pointed at as the requisite for the economical transmission of energy, that I cannot help thinking that, in some cases, a use will be found for electrostatic machines as generators instead of dynamos. Not exactly a Holtz machine—which has a high internal resistance, and must dissipate energy, from its use of non-conductors and sparking intervals—but some gigantic form of Thomson's replenisher with metallic contacts. A large machine is needed to give a current of one ampère, but not outrageously large, and a clear separation of the parts by a millimetre will enable 4,000 volts E.M.F. to be obtained. No great horse-power is here represented, but it would be transmitted by a well insulated wire with very small loss.

In using a waterfall running continuously to light a town for, say, eight hours, batteries are evidently desirable. Three sets might be used, each charging for eight hours—one or other of them always charging, and two of them discharging during the eight hours the light is required. Or two sets would be sufficient if the water-power itself could be used for lighting direct. But the use of three sets of batteries would enable a reasonably distant waterfall to be used, and would, for the eight hours the light is needed, give an actually greater horse-power than the fall itself possesses.

For locomotion in cases where the current cannot easily be supplied to the moving carriage by metallic conduction,

as in tram-cars on common streets, batteries carried by the tram-car afford an obvious, and, moreover, an economical form of motive power. It is sincerely to be hoped that electric tram-cars may speedily come to perfection, and that the excessive wear and tear of horseflesh in this monotonous and severe labor may be checked.

[A number of motors were exhibited, some lent by the Electrical Power Storage Company, and some by Professors Ayrton and Perry. A De-Meritens motor, driven by 18 storage cells, was used to drive a small Siemens' dynamo by a strap, the current from this being used to drive another motor, which worked a lathe, or was used to light four small Swan lamps. A Pilsen arc-lamp was shown, working very steadily with the current from the battery, and a number of Swan lamps were also excited, so as to show how entirely any one was unaffected by the turning out of the others, a battery behaving in this respect like a very good compound wound dynamo. Diagrams of the mode of connecting up cells and dynamos for transmission of power and curves representing the charge and discharge of cells, as tested by Dr. Hopkinson and Messrs. Ayrton and Perry, were also exhibited, but it is thought unnecessary to reproduce them here.]

For driving boats electrically, secondary batteries are obviously necessary, because the laying on of a current by a wire is not only inconvenient but impossible. The storage cells will act as ballast, and can be charged while the boat is moored. For swift short voyages, and for pleasure vessels, nothing better than electrical propulsion is likely to be devised. For long voyages and big ships, the weight of secondary batteries would need all the floating power the ship possesses, and perhaps more. The energy consumed in propelling the present Atlantic liners is something almost incredible.

When motors are driven by secondary batteries with very short connecting wire, the question arises, is high E.M.F. still desirable, as it is when transmission across great distances is concerned, or may powerful currents be now used with advantage? The answer is that, neglecting the resistance of battery and connecting wires, there is neither gain nor loss in using powerful currents, provided the dynamo is properly wound, and has brushes that will not burn up.

But if we do not neglect the resistance of the batteries—a thing we have no right whatever to do—the answer is, that high E.M.F. is of very great advantage. A large number of cells in series, driving a high E.M.F. dynamo at its highest possible speed, and with as small a current as is needed for the power, is by far the most economical arrangement. Now, a screw for a ship has always needed a high speed for economy, and in the old days gearing used to be employed between the steam-engine and the screw, because no high-speed engine was sufficiently economical. I understand that slow engines are still the best, but the disadvantages of gearing are so great that high-speed direct-acting engines are always used. But a high speed is absolutely essential to economy in an electric motor, and no screw, as ordinarily made, would work at such a speed; consequently there is a temptation to use gearing the other way, and to gear down the dynamo to suit the screw. I hope the attempt will be resisted, and high-speed screws made of small size, smooth surfaces, and low pitch, so that the electric motor axle may be coupled direct to the screw shaft. The direct rotatory action of a dynamo used as a motor, as opposed to the oscillating action of a steam-engine, is a great simplifier of mechanism, and will be found extremely convenient.

For all kinds of short transit locomotion, electric power will, probably, prove to be the most suitable, and the improvement in the atmosphere of the Underground Railway, which would result from the adoption of such a means of propulsion, would be far more remarkable than anything which ventilators, whether ornamental or the contrary, are able to effect. In fact, there can be little doubt that the practical development of electrical appliances in the near future will be something extraordinary, and the consequence of this, and of the extensive use of gaseous fuel, will be, I most sin-

cerely hope, and not unreasonably believe, not only the abatement, but the compulsory abolition, of smoke and all other artificial pollution of the atmosphere, and such an improvement in the air of towns as will change them from stifling purgatories into wholesome and refreshing centres of life and work.

THE ALGEBRA OF ALGEBRAIC NUMBERS.

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I.

1. The object of the following paper is the discussion of negative quantities as found in algebra, or rather the finding a logically developed system, which shall include such quantities as special cases, and thus tend to the great generalization of problems and theorems.

In the older algebras the fundamental theorems were established on the supposition that the various letters, used as symbols, denoted pure *arithmetical* or *absolute* numbers, and the results were *assumed* to be general or true for the so-called negative quantities as well as ordinary numbers. But there are so many objections to a method without a rational basis, even though it gives true results, admitting of a proper interpretation, that of late years attempts have been made to substitute an algebraic series of numbers, involving both plus and minus quantities, for the ordinary series, so that a definite meaning may be given to a minus number, and the ordinary algebraic processes be given a rational basis for all cases. We shall endeavor in what follows to deduce the ordinary laws of addition, subtraction, multiplication and division, for such a series, calling especial attention to difficulties which naturally occur to any one in the development of the common system, and pointing out how these difficulties are overcome by the "symbolical algebra," as it has been termed.

It will conduce to a better appreciation of the subject if we first give very briefly the usual method of deducing the ground rules for arithmetical numbers and point out their limitations.

We shall first then regard the letters used as symbols to denote ordinary numbers.

2. *Addition.*—Let it be required to add $(8a-9b)$ to $(5b-4a)$.

It is tacitly assumed that $(8a-9b)$ is positive and denotes a real number, also that $(5b-4a)$ is positive and denotes an ordinary arithmetical number, whole or fractional. That is, we assume that if the proper numerical values are substituted for a and b, that $8a$ is greater than $9b$, and that $5b$ is numerically greater than $4a$.

Now, if we simply add $5b$ to $(8a-9b)$, we have $(5b+8a-9b)$, but this sum is too great by $4a$, as we had to add $5b$ diminished by $4a$, so that we must subtract $4a$ from this first result to get the correct sum, which is therefore,

$$5b+8a-9b-4a.$$

In order to reduce this expression to its fewest terms, we have to make use of the *law*, that is easily proved for numbers, that the order in which we combine the terms is immaterial. Thus, if from $(5b+8a)$ we subtract $9b$, the result is $(8a-4b)$, since we evidently reach the same value by adding $5b$ to $8a$ and then subtracting $9b$, as in simply subtracting $4b$ from $8a$. From $8a-4b$, we have now to subtract $4a$, giving $4a-4b$ for the correct answer.

In practice we set down the terms and add thus:

$$\begin{array}{r} 8a-9b \\ -4a+5b \\ \hline 4a-4b \end{array}$$

But it must be distinctly understood that $-4a$ by itself means nothing, and that we have only written for convenience like terms under each other, so that $(-4a+5b)$ must be interpreted $(5b-4a)$, which is agreeable to the law mentioned.

From a consideration of such examples we deduce the law in addition: Combine like terms by adding those of like sign and prefixing the common sign, and when of unlike signs take the difference of the sum of the positive and the sum of the negative terms and prefix the sign of the greater.

3. *Subtraction.*—Suppose we have to subtract

$$5b-4a \text{ from } 8a-9b.$$

If we take $5b$ from the minuend, the indicated result is

$$8a-9b-5b;$$

but we have taken away too much by $4a$, for we had only to subtract $5b$ less $4a$, so that we must increase this result by $4a$, giving for the correct answer

$$8a-9b-5b+4a=12a-14b.$$

Combining the terms as mentioned above by adding $8a$ and $4a$ and subtracting $14b$, which is the same thing as first subtracting $9b$ and then $5b$. This result is briefly represented thus:

$$\begin{array}{rl} \text{From} & 8a-9b \\ \text{Subtract} & -4a+5b \\ \hline \text{Remainder} = & 12a-14b \end{array}$$

which is thus equivalent to the rule; change the signs of the subtrahend and proceed as in addition.

We again note that the minuend and subtrahend are tacitly assumed to be real positive numbers, whole or fractional, and further that the subtrahend is numerically in value less than the minuend. There is, besides, no sense in subtracting $-4a$ from $+8a$ by itself, for $-4a$ has no existence by itself. Neither can we subtract $5b$ from $-9b$ since the last term is an absurdity by itself in arithmetical algebra. Neither can any meaning be attached to adding the same terms ($-4a$ to $8a$ or $5b$ to $-9b$), in article 2, though we get so accustomed to using the well-known rules in apparently adding or subtracting such single terms that we lose sight of our tacit assumptions, that it is only when combined into two expressions, both numerically positive and in the case of subtraction, the minuend greater than the subtrahend, that we can apply such rules at all.

4. *Multiplication.*

$$(a+b)c=ac+bc$$
$$(a-b)c=ac-bc$$
$$(a+b)(c+d)=(ac+bc)+(ad+bd)$$
$$(a-b)(c+d)=(ac-bc)+(ad-bd)$$

These four results are obvious enough since $(a+b)c$ means the number $(a+b)$, repeated c times, and is equal to a (one part), repeated c times, added to b (the other part), repeated c times. Similarly we go through. In the next case we have to multiply $(a+b)$ by $(c-d)$ or repeat $(a+b)$ c times and diminish the product by $(a+b)$ repeated d times

$$\therefore (a+b)(c-d)=ac+bc-(ad+bd)$$
$$=ac+bc-ad-bd$$

Similarly,

$$(a-b)(c-d)=ac-bc-(ad-bd)$$
$$=ac-bc-ad+bd$$

Now, in place of going through the successive steps given here, we shall reach the same result by multiplying each term of the multiplicand by each term of the multiplier, noting a rule that *like* (prefixed) *signs give plus, unlike minus,* in the product. It is again tacitly assumed that both multiplicand and multiplier are positive (arithmetical) numbers.

There is no sense, *per se*, in

$$-b\times c=-bc$$

or in

$$-b\times -d=+bd$$

It is only in connection with the other terms that such results have any meaning so far as expressions containing only arithmetical numbers are concerned.

5. *Division.*—In division the aim is to find a factor called *quotient* that multiplied by the divisor will give the dividend, so that the rules of signs and limitations hold as in multiplication.

6. We see, therefore, that with such an *algebra of ordinary numbers,* connected by addition and subtraction signs, that we are very much circumscribed, for we have seen that the expressions we treat must be essentially positive, whereas, in numerous investigations, it is impossible to say whether all the expressions we combine by addition, subtraction, multiplication or division, are positive or negative, so that we are, perforce, obliged to apply our rules to negative quantities

without having defined them or proved any rules with respect to them.

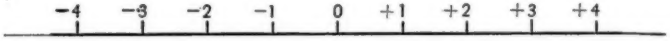
In certain problems, too, the unknown quantity is found with a negative sign before it, and we have to go back and vary the statement of the problem to make the result intelligible, when that is possible.

The great value that lies in the conception of the true, real, negative quantities, consists in the wonderful generality given to general formulæ, which may be deduced, say, on the supposition that all the letters denote plus quantities, when a simple substitution in the final result of the actual values (in any case) of the letters (for that case) will give a correct formula.

In the applications of algebra to geometry we shall find still stronger reasons for an early consideration of + and — quantities.

We thus see that it is not only necessary in certain cases but highly important that the greatest generality should be given to algebraic processes, and we shall now proceed to the consideration of the algebraic series of numbers, which, together with an extension of meaning given to the signs plus and minus, will enable us to give the generality desired.

7. The algebraic system of numbers is supposed to increase from zero in the opposite directions as in the scale below.



Those numbers to which are prefixed the + sign are called positive numbers, those with the negative sign minus numbers; so that $(-a)$ means a quantity or number of *real* units, but measured in a direction opposite to the positive one. There is no meaning in the statement that $(-a)$ is "less than zero" without a new definition of "less than," so let the reader bear in mind that in dealing with $(-a)$ he is dealing with a real arithmetical number, though taken in the opposite sense to $(+a)$.

For the present it is well to indicate an algebraic number by enclosing it in a parenthesis.

The + and — quantities above may be conceived as measuring distances right and left from 0, so that the line drawn will be considered as divided into equal parts and may be called the scale of numbers. In this case direction to the right is called +, that to the left —, but we can just as well assume the positive direction to the left when the negative way of counting will be to the right.

As illustrations of quantities having opposite directions, we may mention motion forward and backward, or degrees above and below zero on a thermometric scale. Thus, if a moving point, starting at 0 in the scale, moves 5 units of distance to the right and 9 to the left, we reach (−4) on the scale which thus indicates that the final position of the moving point is 4 units to the left of the starting point or zero of the scale.

Again, if it moves 5 units forward, then 9 units backwards, then 6 units forward, its final position will be 2 units from 0 in a forward direction, or to the place denoted by (+2). If direction forwards is called +, then direction backwards will be — as we see illustrated in this case.

8. *Addition of Algebraic numbers.—Rule.* To add two algebraic numbers, *we start at the point in the series indicated by the first number and count in the direction given by the sign of the second number the number of absolute units in that number.*

This operation is indicated by putting the + sign between the algebraic numbers; thus to add (+3) to (—5) we start at (+3) in the scale and count in the negative direction 5 units, thus bringing us to (—2). The symbolical representation is as follows:

$$(+3)+(-5)=(-2).$$

Similarly, in "adding" any number of algebraic numbers, we add the first two as just explained, then the third to the previous result by the same rule, and so on.

Thus,

$$(+3)+(-5)+(+3)=(+1).$$

Here we start at (+3), go 5 units to left to (—2), then 3 units to right to (+1), the result of the *algebraic addition*.

We see that the rule for adding two algebraic numbers is consistent with the

definition, *that* $(-a)$ *means absolute units measured in the opposite direction on the scale of numbers to the plus direction, so that when combined by the sign of addition with* $(+a)$, *the result is zero.*

9. By the consideration of examples like the following:

$$(+3)+(+2)=(+5);$$
$$(-3)+(+2)=(-1);$$
$$(+3)+(-2)=(-1);$$
$$(-3)+(-2)=(-5;$$

We easily deduce the rules:

I. To add two numbers with *like* signs, find the arithmetical sum of their absolute values and prefix the common sign to the sum.

II. To add two numbers with unlike signs, find the arithmetical difference of their absolute values and prefix the sign of the greater.

Where there are many numbers to add of different signs, we place the + numbers in one column, and the − numbers in another column, add separately and combine the sums. It is evident that the result is the same in whatever order we add algebraic numbers, since the total number of units we go to the right, by the successive steps, equals the sum of the absolute values of the positive numbers. Similarly for the minus numbers; so that the final position is given by the relation between the two sums.

10. Let a and b denote the absolute values of any two numbers, then the four cases given in article 9 can be generalized and written in the following forms.

$$(+a)+(+b)=a+b;$$
$$(-a)+(+b)=-a+b.$$
$$(+a)+(-b)=a-b;$$
$$(-a)+-b)=-a-b.$$

The interpretation to be given to the right members must follow from some convention, and this convention is, that when a quantity, as a, *has no sign before it, the plus sign must be understood;* so that a is the same thing as the algebraic number $(+a)$; likewise, that *if a quantity* a *has* a + *or* − *sign before it, that sign will indicate whether it is* a + *or* − *algebraic number,* to be combined by addition with any other numbers in the same expression.

Thus, $a-b$ is the same as $(+a)+(-b)$, and $-a-b$ the same as $(-a)+(-b)$.

This convention enables us to dispense with parentheses and a multiplicity of signs when desired; though for the sake of clearness we shall retain them in establishing fundamental rules, whilst indicating, at the same time, abreviated forms. However, if an expression is given in the abbreviated forms, shown by the right members of the above equations, we can always express it in the manner shown by the left members, when desirable.

II. We will now show how the use of algebraic numbers enables us to generalize by expressing all the cases of a certain problem by one equation.

We must always premise by stating what class of quantities are to be regarded as +, and which (those opposed to the + one) minus. Thus, in the case of the moving body in art. 7, let us count distances forward +, those backward, minus.

Then, if a person walks say 5 feet forwards from a given origin, then 9 feet backwards along the same straight line produced when necessary, we can say he moves successively +5 and −9 feet, thus stopping 4 feet back of the origin, or at −4. Thus, if we call X his final distance in feet from the origin, + if forwards, − if backwards, we can write,

$$X=(+5)+(-9)=(-4),$$

or, more simply,

$$X=5-9=-4.$$

Again, if the person moves 5 feet forwards, then 9 backwards, then 6 forwards, we have his ultimate position,

$$X=5-9+6=+2;$$

or two feet in front of the origin.

Now, no matter how many movements forwards and backwards are made, we see that we can state generally that the final position of the moving body is the algebraic sum of his successive movements, so that,

$$X=(a)+(b)+(c)+\ \cdot\ \cdot\ \cdot$$

where a, b, c, . . . represent the successive movements which are essentially negative when backwards, otherwise positive, as written.

Again, as to dates, let dates reckoned

after Christ be designated as +, those anterior −, and let any interval of time be regarded as + if reckoned forwards, − if reckoned backwards; then having given a date D and the interval I to another date D′, we can find the letter by the formula,

$$D+I=D'.$$

What is the date 20 years after 240 B.C.?

$$(-240)+(20)=-220,$$

or 220 B.C.

What is the date 20 years before 240 B.C.?

$$(-240)+(-20)=-260,$$

or 260 B.C.

What is the date 40 years after 20 B.C.?

$$(-20)+(+40)=(+20),$$ or 20 A.D.

Julius Cæsar died A.D. 14, aged 77 years; when was he born?

$$(+14)+(-77)=-63,$$ or 63 B.C.

Similarly for any series of successive intervals we shall always have,

$$D+\text{sum of intervals}=D'.$$

Thus one formula comprehends every case. Other illustrations of such generalizations can be made in the case of quantities that stand in opposition, as *assets* and *debts*, *gain* and *loss*, &c. The algebraic sum of assets taken as plus, and sum of debts as minus, gives total assets if the result is +, or total indebtedness if −, &c.

Examples of a far more complicated character will be given further on, as we deduce rules for subtraction, multiplication, &c., of algebraic numbers.

Subtraction.

12. We have previously only used the + sign before an *algebraic* number, having clearly defined the process of addition intended by it. We shall similarly give a definition of a *minus sign* before an *algebraic number*, consistent with the use already made of it to indicate a negative number, and will remark that everything that follows will be strictly deduced from the consequences of these definitions.

Definition.—A minus sign before an algebraic number indicates that the number is to be counted in an opposite direction to that indicated by its sign, or it indicates a reversal of direction of the line representing that number.

Thus,

$$-(+5)=(-5),$$
$$-(-5)=(+5).$$

This process involves no absurdity for single numbers; for the results (−5) and (+5) in the two cases are perfectly intelligible; meaning algebraic numbers, the one plus, the other minus, that will cancel each other if added together. From the definition, a − sign before an arithmetical number causes it to be counted in the opposite direction to the usual or + direction, as the number may be regarded as a + number.

13. When this minus sign is placed between two numbers, as $(+8)-(+5)$, it indicates what is called *algebraic subtraction*. The quantity before which it is placed is called the subtrahend, the other one the minuend.

From the definition, it follows that to subtract one quantity from another, *we change the sign of the subtrahend and add.*

Thus,

$$(+3)-(+1)=3-1=2,$$
$$(+3)-(-2)=3+2=5,$$
$$(-3)-(+1)=(-3)+(-1)=(-4),$$
$$(-3)-(-1)=(-3)+(+1)=(-2).$$

We have left the parentheses around certain minus quantities in the right numbers, though it is not necessary if we attend to the meaning given in art. 10.

It is seen from a consideration of these cases that the *absolute number* given as the result of the algebraic subtraction of two algebraic numbers, *gives their distance apart on the scale of numbers, and that the prefixed sign is the direction from the subtrahend to the minuend.* Hence the result is called the *algebraic difference.*

As in art. 10, we can write briefly,

$$(+a)-(+b)=a-b\,;\qquad (-a)-(+b)=-a-b,$$
$$(+a)-(-b)=a+b\,;\qquad (-a)-(-b)=-a+b.$$

In the right numbers of these equations the a's and b's are to be taken with the sign prefixed to each, or understood according to the convention given

in art. 10, and added by the rules given in art. 9.

14. We see from the foregoing that we employ the same rules exactly as in addition and subtraction of polynomials in the arithmetical algebra, and that when + single numbers are taken, the algebraic sum or difference of any two of them is exactly as in the arithmetical algebra. As before explained, arithmetical algebra cannot handle single minus numbers or expressions that are essentially minus; but now there are no limitations as to signs, for the operations on negative quantities are just as intelligible and as clearly defined as on positive quantities.

15. On comparing the formulæ just given in art. 13 with those in art. 10, we notice the relation:

$$(+a)+(+b)=(+a)-(-b),$$
$$(+a)+(-b)=(+a)-(+b),$$

or

$$a+b=a-(-b),$$
$$a+(-b)=a-(+b)=a-b.$$

So that algebraic additions can always be written as algebraic subtraction by changing the sign of the term, which is to appear as the subtrahend. We can thus go from one form to the other at pleasure. In fact, from the comprehensive definition given to a minus sign before an algebraic number (article 12), we can write,

$$a+b=a-(-b)=a-[-(+b)]$$
$$=a-[-(-(-b))], \text{ etc.}$$

Similarly,

$$a-b=a-[-(-b)]$$
$$=a-[-\{-(-(-b))\}].$$

There is thus choice in the form in which a given value can be expressed.

16. The principles deduced for algebraic numbers apply equally to algebraic expressions, composed of several numbers combined in any way, since, if the numerical values are assigned to the letters and the indicated operations performed, the expression reduces to a single number when the rules apply.

Thus to subtract $5b-4a$ from $8a-9b$, we have,

$$8a-9b-(5b-4a)=$$
$$8a-9b-5b+4a=$$
$$12a-14b.$$

Compare the result in art. 3, where it was premised that $8a$ was greater than $9b$, and that $5b$ was greater than $4a$, also that the subtrahend $(5b-4a)$ was less than the minuend $(8a-9b)$. By the use of algebraic numbers there are no limitations of this kind whatever. As in this example, we shall generally, in what follows, leave off the parentheses to indicate the algebraic numbers. From a consideration of the results in art. 15, the expressions can be transformed so as to contain them when desired.

Multiplication.

17. In multiplying any number by a, we understand that the number is to be repeated a times, so that we only have to define what is meant by multiplying by $+a$ or $-a$; in other words the significance of the + or − signs before a multiplier.

Now, *since* a + sign before an algebraic number does not reverse the direction of counting (art. 10), and a −sign does (art. 12), we shall define,

1. Multiplication of an algebraic number by $(+a)$ means that the number is to be repeated a times, the direction of counting being unchanged.

2. Multiplication of an algebraic number by $(-a)$ signifying that the number is to be repeated a times, and the direction of counting reversed.

Thus,

$$(+a)\times(+b)=+ab,$$
$$(-a)\times(-b)=+ab,$$
$$(+a)\times(-b)=-ab,$$
$$(-a)\times(+b)=-ab.$$

From the first two results we deduce the rule, "*like signs give plus*," and from the last two the rule, that "*unlike signs give minus.*"

18. Since *division* may be defined as finding the factor called quotient, when the other factor (the divisor), and the product of the two factors, called the dividend, is given, it is seen that the same rules for signs hold for division.

Thus,

$$\frac{(+ab)}{+b}=a;\ \frac{(+ab)}{-b}=-a;$$
$$\frac{(-ab)}{-b}+a;\ \frac{(-ab)}{+b}=-a.$$

19. Since a fraction may be considered an indicated quotient, the same laws as to signs hold for fractions.

20. Having defined multiplication of single numbers by plus and minus quantities, we must now ascertain if these rules can be utilized for the multiplication of polynomials by polynomials.

Now,

$$(a-b)n=(a-b)+(a+b) \ldots n \text{ times},$$
$$=a+a+ \ldots n \text{ times}, -b-b- \ldots n \text{ times},$$
$$=na-nb.$$

This is called the distributive law. Similarly, to multiply $(a-b)$ by $-n$, we get the same result by *definition*, except that the direction of counting of the result is changed, so that

$$(a-b)\times -n=-na+nb.$$

Hence the rule: multiply each term of the polynomial by the monomial, according to the laws laid down for single numbers, and add the results. Next suppose the multiplier, $(+n)$ or $(-n)$, is itself expressed as a polynomial. Thus, if 10 is to be multiplied by $8-3$ or 5, it is evident that we get the same result by repeating the 10 8 times in the positive direction and 3 times in the negative direction, and adding the results as by multiplying the 10 by 5.

This operation is recorded thus:

$$\begin{array}{r} 10 \\ +8-3 \\ \hline +80-30=50 \end{array}$$

That is we multiply the multiplicand by each term of the multiplier according to the laws for single numbers, and add the partial products.

Similarly we multiply 10 by $-8+3$ or -5,

$$\begin{array}{r} 10 \\ -8+3 \\ \hline -80+30=-50 \end{array}$$

Here the direction of the 10 is reversed on multiplying by -8, giving -80; but not reversed on multiplying by the $+3$, which gives $+30$, which added to -80 gives -50; exactly what we should obtain by repeating the 10, 5 times in the negative direction. There is no difference in principle in multiplying a negative multiplicand by a polynomial.

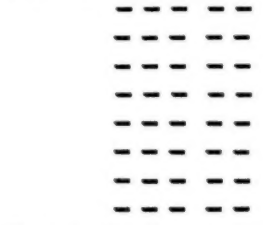
In fact we are not limited, as in the case of arithmetical numbers, but both multiplicand and multiplier can be essentially negative, and the results are as perfectly intelligible as in the case of absolute numbers. The laws just deduced, by combination enable us to apply the usual rules to the multiplication of any polynomial by another polynomial. Thus to multiply $(-a-b)$ by $(c-d)$, we obtain,

$$\begin{array}{r} -a-b \\ c-d \\ \hline -ac-bc+ad+bd. \end{array}$$

That is, we multiply the $(-a-b)$ first by c, by the distributive law, without reversing, and then by d, reverse the last result, and combine with the other.

21. The rules for the division of polynomials by polynomials, are deduced from these in the usual manner.

22. The rule for multiplying by a polynomial may be illustrated graphically as follows: Let it be required to multiply 8 by $(3+2)$, or generally m by $(a+b)$.



There are 8 units in each vertical row, and 8×3 and 8×2 units in the two columns, making $8\times5=8\,(3+2)$ units in all.

$$\therefore 8(3+2)=8\times3+8\times2.$$

Similarly,

$$8(5-2)=8\times5-8\times2;$$

for 8×5 gives the whole number of units and $8+2$ those of the right column, so that their difference equals the number in the left column $=8\times3=8(5-2)$.

23. We shall note a special case of multiplication, $(-a)^2$ as the form often occurs. Now by the rule, article 17, we have

$$(-a)^2=(-a)\times(-a)=\times a^2-$$

for $(-a)^3$, means that $(-a)$ is to be repeated a times and the direction of counting reversed.

If this result is again multiplied by $-a$, by the same rule, we obtain,

$$(-a)^3=+a^2\times-a=-a^3$$

Similarly

$$(-a)^4=(-a^3)\times-a=+a^4;$$

so that a negative quantity raised to an even power gives a + result; to an odd power a − result.

24. We shall group together some of the fundamental formulae so far deduced, and note an important fact resulting from the comprehensiveness of the definitions relating to the influence of signs.

The following formulae have been drawn from articles 10, 13, 17 and 18; and it is to be observed that the left members indicate the addition, subtraction, multiplication and division of algebraic numbers, and the right members are intended to show the indicated operation *performed*, as far as this can be done where numerical values are not assigned to a and b.

We have put parentheses around the algebraic numbers, in the left members, as before, for without them, in the case of $a-b$, we might be in doubt whether we are to include it under the rule of addition and write it, $(+a)+(-b)$ or to consider it a case of subtraction and write it $(+a)-(+b)$. The parentheses around the plus quantities as well as the sign + enclosed, can be omitted, if desired, since a quantity is assumed as +, when it has no sign before it, so that $a+b$ would generally be understood as $(+a)+(+b)$; but as it fixes the attention more precisely upon the algebraic operations involved, the parentheses were retained in the left members throughout.

The right members are, of course, algebraic members, only under a different form from the left numbers (see article 10), the results of the combinations, according to the rules, in both members, being precisely identical.

The formulae are as follows:

$$1.\quad \begin{cases} (+a)+(+b)=a+b; & (-a)+(+b)=-a+b; \\ (+a)+(-b)=a-b; & (-a)+(-b)=-a-b; \end{cases}$$

$$2.\quad \begin{cases} (+a)-(+b)=a-b; & (-a)-(+b)=-a-b; \\ (+a)-(-b)=a+b; & (-a)-(-b)=-a+b; \end{cases}$$

$$3.\quad \begin{cases} (+a)\times+b=+ab; & (+a)\times-b=-ab; \\ (-a)\times-b=+ab; & (-a)\times+b=-ab; \end{cases}$$

$$4.\quad \begin{cases} \dfrac{(+ab)}{+b}=a; & \dfrac{(+ab)}{-b}=-a; \\ \dfrac{(-ab)}{-b}=a; & \dfrac{(-ab)}{+b}=-a. \end{cases}$$

Now note, that if in the left members we substitute $(-a)$ everywhere for a and perform the indicated operations, as far as may be, that the result is precisely the same as if we substitute $(-a)$ for a in the corresponding right members and reduce according to the laws established.

Thus put $(-a)$ for a in both members of the first formulae of each series, we obtain

$$(-a)+(+b)=-a+b$$
$$(-a)-(+b)=-a-b$$
$$(-a)\times(+b)=+(-a)b=-ab$$
$$\frac{(-a)b}{+b}=(-a)=-a.$$

Thus having deduced a formula, as $a\times b=ab$, we can ascertain the result for $(-a)\times b$ by substituting $(-a)$ for a in the right member, or performed multiplication, giving $(-a)b=-ab$.

25. Again to multiply $(a+b)$ by a, we indicate the process as follows:

$$\begin{array}{r} a+b \\ a \\ \hline a^2+ab \end{array}$$

This may be called a general formula, true whether b is + or −; for if b is minus, so that we have to multiply $a+(-b)$ by a, we have only to change b to $(-b)$ in the result and reduce by the laws of signs, giving the correct answer, a^2-ab. This follows from the principle just noted.

In the formula,

$$n(a+b+c)=an+bn+cn,$$

if we put $a=-x$, $b=-y$, in both numbers, the results must be identical by the principle or law that was found to exist.

$\therefore n(-x-y+c) = -xn-yn+cn.$

In the multiplication or division of polynomials by polynomials, the same general principle as to substitution exists, for the results are derived from the multiplication, division, addition and subtraction of monomials according to the rules affecting single numbers.

Thus by actual multiplication, we deduce the formula,

$$(a+b)^2 = a^2+2ab+b^2$$

By substituting $(-b)$ for b in this formula, both sides, and reducing, we find the correct formula,

$$(a-)^2 = a^2-2ab+b^2.$$

We can put the actual multiplications side by side to make this plainer:

$a+b$	$a+(-b)$
$a+b$	$a+(-b)$
a^2+ab	$a^2+(-ab)$
$+ab+b^2$	$+(-ab)+(-b)^2$
$a^2+2ab+b^2$	$a^2-2ab+b^2$

Thus if in the partial product $(a+b)\times a = a^2+ab$ we substitute $(-b)$ for b, we get the first partial product on the right. Similarly for $(a+b)\times b$. Now since, by article 15, we can write $+(-ab) = -ab$, and by article 23, $(-b)^2 = +b^2$, the result on the right is a^2-ab+b^2, just what we find by putting $(-b)$ for b in the final result on the left.

Now it is readily seen that all this follows from the general principle enunciated above, so that it is not necessary to analyze every case in this way, but we may consider that the principle of substituting $(-a)$ for a on both sides of any formula giving identical results as established.

Again having deduced the general formula,

$$(a+b+c)^2 = a^2+b^2+c^2+2ab+2ac+2bc,$$

if in any particular example b and c say are minus, we have at once without the necessity of multiplication, by substitution only,

$$(a-b-c)^2 = a^2+b^2+c^2-2ab-2ac+2bc.$$

26. The principle in question is likewise evident if the minus quantity is always kept in a parenthesis, thus simply taking the place of the corresponding + quantity throughout. Thus to cube $(a-b)$, we may multiply as follows:

$$\begin{array}{l} a+(-b) \\ a+(-b) \\ \hline a^2+a(-b) \\ \quad +a(-b)+(-b)^2 \\ \hline a^2+2a(-b)+(-b)^2 \\ a+(-b) \\ \hline a^3+2a^2(-b)+a(-b)^2 \\ \quad +a^2(-b)+2a(-b)^2+(-b)^3 \\ \hline a^3+3a^2(-b)+3a(-b)^2+(-b)^3 \\ = a^3-3a^2b+3ab^2-b^3. \end{array}$$

If we had formed the product $(a+b)^3$ along side of this, we should have noted the identity throughout, except that b is everywhere simply replaced by $(-b)$, so that the final result can be obtained from that pertaining to $(a+b)^3$ by simply changing b to $(-b)$ and reducing according to the laws of signs.

Similarly from the developments of $(a+b)^n$, we deduce the formula for

$$(a-b)^n = a^n - na^{n-1}b + \frac{n(n-1)}{1.2}a^{n-2}b^2 - \&c.$$

27. Numberless illustrations may be given of the application of the rule. We shall content ourselves with mentioning two more general formulæ: the first pertaining to simple quadratic equations, the other to simultaneous equations of the first degree with two unknown quantities.

From the general equation of the 2d degree

$$x^2+px+q = o,$$

we deduce

$$x = -\frac{p}{2} \pm \frac{1}{2}\sqrt{p^2-4q}$$

Now if we have given the equation

$$x^2-x-20 = 0,$$

on comparing it with the first one we note that

$$p = (-1) \text{ and } q = (-20),$$

hence, substituting these values in the expression for x, we find the value in this case.

$$\therefore\ x=-\frac{(-1)}{2}\pm\frac{1}{2}\sqrt{(-1)^2-4(-20)}$$

$$\therefore\ x=\tfrac{1}{2}\pm\tfrac{9}{2}=5 \text{ or } -4.$$

Again take the equations

$$\left.\begin{aligned} ax+by&=c\\ a'x+b'y&=c'\end{aligned}\right\}$$

By elimination we obtain

$$x=\frac{cb'-bc'}{ab'-ba'}$$

$$y=\frac{ac'-ca'}{ab'-ba'}$$

In case any of the numbers a, b, c, a', b', c' are minus, in any particular example, we can at once write the values of x and y from the last two equations by a simple substitution, without the necessity of an actual elimination process.

This is evident, because in this process in the general case we should have only to substitute $(-a)$ for a, etc., throughout, to get the corresponding expressions where a, . . . were minus.

We have now deduced all the fundamental rules of algebra from a few comprehensive definitions relating to the meaning of + and − signs in connection with numbers, combined in various ways, and have shown the wonderful generality given to formulæ by the use of the algebraic negative quantity.

28. We shall now discuss some problems where we shall similarly reach the greatest generality by the same means. In fact the use of negative quantities is necessary in certain problems, where although $(a-b)$, to take an illustration, may be assumed as plus at the beginning, yet it may turn out in the end that $a<b$, so that we have supposed an impossible subtraction if a and b are regarded as absolute numbers. By the use of the algebraic series, however, all difficulty disappears, not only in the work but also in the interpretation of the answer when minus; though here it is necessary to state in the beginning the positive sense, when the negative will be just the reverse.

29. *Problem* 1.—A's age is now a years, and B's age is now b years, how many years before or after this epoch to the period when A's age is m times B's.

Let x=number of years, regarded as + if reckoned in the future, − if in the past; then in any case

$$a+x=m(b+x).$$

We see that if x turns out to be plus, the formula is correctly stated, as well as when x is minus, or when the period antedates the present.

Solving we at once find

$$x=\frac{a-mb}{m-1}$$

Now, if $a=40$, $b=15$, $m=2$, then $x=+10$ years; *i. e.*, in 10 years A's age will be double B's.

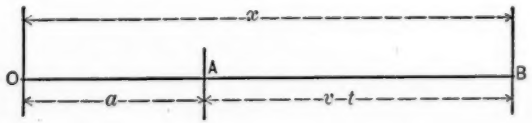
But if $a=35$, $b=20$, $m=2$, then $x=-5$, that is, A was twice as old as B 5 years ago. It is not necessary to make a new statement of the problem, so that x shall come out +, as is generally done. The result is perfectly intelligible as it stands.

30. *Problem* 2.—The celebrated problem of the couriers. Two couriers, at a certain instant of time, are at certain points on a straight road, and moving towards or from each other at certain uniform velocities. When were they together, or when will they be together?

It seems impossible, at first glance, that one solution can cover all the cases that can be imagined, but we shall find it to be so.

In the figure below the first courier is supposed at the point A, distant a from O, *the same instant* the second courier is at some other point A′ (not shown in the figure), distant a' from O.

Now, the two couriers are supposed to meet at B, distant x from O, either t hours before or t hours after passing the two points A and A′; in the first case t will be called minus, in the second case plus. Similarly,



x, a and a' will be + if measured to the right of O, otherwise −.

The rate per hour, or velocities v and v', at which the couriers travel, will be called + if the motion is to the right, − if to the left in either case.

We are thus carrying out principles already employed in article 11, that distance to the right will be called +, if distance to the left is minus; time after is +, if time before is −, and motion to the right + when motion to the left is called minus.

Where all the quantities are plus, we can at once write the formula for the first courier,

$$x=a+vt \quad \ldots\ldots \quad (1).$$

But, by art. 11, this formula holds in all cases, for $x=a+vt$ is the algebraic sum of two numbers, and thus gives the distance of the courier from the origin O at the time of meeting, this distance being measured to the left if x is minus, otherwise to the right.

This can be made plain by noting that if v is + and t +, the product vt=distance AB is +; if the courier is going to right still, but he met the other one + hours before reaching A, then + is −, and vt is −, as it should be, since B is to the left of A.

Now, for the two cases where the motion is to the left, or v minus, t may be + or −; if t is +, or the time of meeting was t hours after the courier passed A going to the left, then the term vt will be minus in the formula; when t is − also, vt will be plus, for now the point of meeting is to the right of A.

The reader can readily illustrate the various cases, dependent upon the signs of a, v and t, by diagrams. For the second courier, we should have the analogous formula for the distance OB,

$$x=a'+v't \quad \ldots\ldots \quad (2),$$

whence, placing the two values of x equal, we deduce,

$$t=\frac{a'-a}{v-v'} \quad \ldots \quad (3),$$

$$x=\frac{va'-av'}{v-v'} \quad \ldots \quad (4).$$

At the same instant of time the two couriers are respectively at the distances a and a' from O, so that $(a'-a)$ in (3) represents their distance apart at that instant (article 13). Likewise $(v-v')$ is the arithmetical difference or sum of their velocities according as they are going in the same or opposite directions; hence we know eq. (3) to be correct independently of previous considerations.

As a particular case, let $a'=10$ miles, $a=(-20)$ miles, $v=4$, and $v'=6$ miles per hour, then from (3) we find,

$$t=\frac{10-(-20)}{4-6}=\frac{30}{-2}=-15.$$

That is, they met 15 hours before the time first courier was at A, distant 20 miles to the left of O.

Now, in 15 hours the first courier travels $4\times15=60$ miles, so that they met $60+20$ or 80 miles to left of O, which we likewise find from formula (4),

$$\therefore\ x=\frac{4\times10-(-20)6}{-2}=-80.$$

For the special cases when $v=v'$, (3) gives,

$$t=\frac{a'-a}{o}=\infty;$$

or they will never meet, as is evident, since the assumption is, they have the same velocity and are going in the same direction.

If, besides, $a'-a=o$, or they are together at the start, they will be together at all times, so that t can be anything. Here the equation takes the singular form,

$$t=\frac{o}{o}$$

31. We have now given the theory and a sufficient number of illustrations of the use of the algebra of algebraic numbers to show the generality obtained by its use. The theory has been developed by giving comprehensive meanings to the signs + and −, so as to include arithmetical algebra as a special case when the numbers or symbols representing them are all plus quantities.

It is not proposed to develop the theory further, except to note an extension of meaning given to "greater than" or "less than," with a necessary caution.

Thus an algebraic quantity is said to be *greater than* another when it lies in the positive direction from it.

Thus, we say that $5<-1$, and $-2<$

-3. This is a very useful convention in generalizing results, but it must not be inferred from it that there is such a thing as a *ratio* between a + and a − quantity.

Before the algebraic series of numbers, with the use to be made of it, had been brought forward, there was considerable difficulty in the comprehension of negative quantities as they appeared in analytical investigations, especially where a minus quantity was defined or understood, in some fashion, to be less than zero.

Thus Carnot follows D'Alembert in quashing the whole business as follows:

Consider the proportion.

$$1 : -1 : : -1 : 1$$

If -1, is less than zero, then we have a greater is to a less as a less is to a greater, which is an absurdity ; and yet the product of the extremes is equal to the product of the means, so that the four quantities should be in proportion apparently. Q. E. D.

But we have tacitly assumed here that there is a ratio of 1 to -1, and it is just here that the absurdity is introduced, for from the original definitions of $\times$ and $-$ quantities we see that they are of such opposite character that they can have no ratio, though by the technical rule for division, (which is deduced from that for multiplication) the quotient $\frac{1}{-\frac{1}{1}} = (\frac{1}{1}$ and the direction reversed) giving -1 for the result, is the same as $-\frac{1}{1}$.

If we choose to *extend* the ordinary arithmetical meaning of *ratio* to include algebraic numbers, well and good; but with such merely conventional definitions, it would be next in order to *ascertain*, if in any proportion, we must always have,

$$\text{greater} : \text{less} : : \text{greater} : \text{less}.$$

And we should soon find, by the consideration of this very example in fact, that such a criterion does not exist.

32. This example should serve as a valuable caution to the student, against assuming anything that may turn out to be inconsistent with the definitions. The algebraic series of numbers is essentially much more comprehensive, in most respects, than the natural series, so that in using it as the basis of an algebraic system, we should be all the more careful to give few and comprehensive definitions that are consistent throughout, and deduce the laws of the new algebra entirely from these definitions and obvious elementary principles.

I have fault to find with writers of text books in this very respect, that a number of definitions or rules pertaining to the fundamental operations are given, which to the student seem not to have the proper connection, or in fact may be conflicting; so that he cannot give his full assent, or at least reserves his judgment until the whole ground can be carefully reviewed and the bearing of each case upon every other closely analyzed.

We have endeavored, in what precedes, to deduce the laws from obvious principles, after giving precise, though comprehensive definitions of the meaning of + and − whenever used, whether in multiplication or division, addition or subtraction.

There must be but one meaning to each of these signs, and that meaning is very simple: plus meaning counting forwards, or in the usual direction; minus counting backwards or in the reverse direction. And yet from these simple definitions, we devise the algebraic series of numbers; and then deduce, with definitions consistent with the first, all the fundamental ground rules of addition, subtraction, multiplication and division for both simple and compound expressions.

Thus we have not to give new definitions of + and − under the various heads enumerated, and perhaps leave the student to find out if these meanings be consistent, but we agree in the first instance that wherever + or − occur, we shall understand but one meaning to each of them. Then there can be no inconsistency; for a sign, as −, before either a single term, a product, a quotient or any compound expression, means simply but one thing, that the indicated result must be counted in the reverse direction to that assumed for the positive direction.

Again, supposing the ground rules established in a logical manner, illustrations should follow, without which the student of algebra often wonders why isolated negative quantities are introduced at all; and then again whilst the

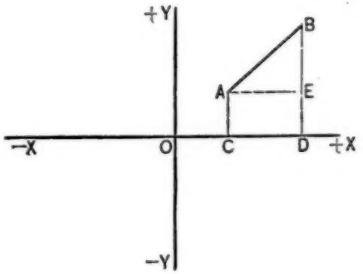
principle of substitution in a general formula, as illustrated in previous articles, is universally made use of, yet I am unacquainted with any attempt at demonstrating the universal correctness of the process.

The truth is the books give too little, and that in too disconnected a form, for the student unaided, to grasp the entire subject. Especially is this true in the transition from multiplication of simple to that of compound expressions.

33. It would lead us far beyond the scope of this article to take up the numberless applications of the algebraic numbers to trigonometry and the higher analysis. Besides in trigonometry, modern text books have already shown the generality of all the formulae by the use of negative numbers, so that little remains to be said. In analytical geometry, though, there is a singular lack in demonstrations going to show the universality of the formulae deduced, to all angles and quadrants; and yet this very generality is often assumed, as if either, sufficiently evident, or following from some sort of general principle previously demonstrated. Now, neither of these suppositions are correct. No student finds the generality assumed, either self-evident or following from principles already demonstrated. In fact, it is absolutely necessary to show, *for each case*, that the formulae deduced is general, for problems can easily be given, in the application of algebra to geometry, where this generality cannot be attained. This is an important difference between purely algebraic formulae and those used in the solution of some geometrical problem. Further, it will often happen that a formulae deduced for a certain case, may include other cases of a different kind, where the algebraical transformations have been such as to increase the number of the roots, over those originally contemplated. Thus in squaring an equation and then reducing and solving, we can introduce double the number of roots intended originally; but generally the false solutions are easily detected.

34. It will be found, too, as a rule, that the generality of the formulæ of analytical geometry and trigonometry rests on the tacit assumption that the rules of signs, as laid down in algebra, are to be observed, although there may be no meaning attached to some of the operations in themselves in certain cases. To make my meaning clear to the students of analytical geometry, let us find the analytical expression for the distance between two points whose co-ordinates are respectively x_1, y_1, and x_2, y_2.

Let A and B in the figure, represent the points, so that drawing AC and BD parallel to OY, we have $OC=x_1$, $AC=y_1$; $OD=x_2$, $BD=y_2$.



Then, by the theorem that "the square on the hypotenuse of a right triangle is equivalent to the sum of the squares on the other two sides," we have,

$$\overline{AB}^2=(x_2-x_1)^2+(y_2-y_1)^2.$$

To ascertain if this formula is general or true, no matter what the co-ordinates x and y, let us first suppose y_1 and y_2 to remain constant, so that A and B will both be above the axis of X, and remain at the same distance from it. Then if B lies to the left of A in the first quadrant, (x_2-x_1) will be negative, but its square $(x_2^2-2x_2x_1+x_1^2)$ will be the same as before, so that the formula is unchanged.

Again, if A and B are both to the left of OY, then x_1 and x_2 are both minus, but $(x_2-x_1)^2$ is equivalent to the square of the projection of AB on OX, as in the former case. Similarly, when A and B are on different sides of OY; for now (x_2-x_1) will no longer be an arithmetical difference, but a sum, or minus a sum, which is the projection of AB on OX, so that $(x_2-x_1)^2$ will be equal to the square of this projection, as before. It is, moreover, evident that the term $(x_2-x_1)^2$ will remain of the same form whether A and B are both above, both below, or one above and the other below OX.

We can prove in an exactly similar manner that $\overline{BE}^2=(y_2-y_1)^2$, no matter in what quadrants A and B may be found, or what the relative values of y_2 and y_1. So that the *one* formula is true for every supposable case, with the understanding, implied as it is, that the rules of signs as deduced in algebra are in force here, and of course the signs of x and y as given in analytical geometry. Thus, to take a numerical example, let

$$x_2=-5,\ x_1=+2,\ y_2=+4,\ y_1=+6,$$

then,

$$\overline{AB}^2=(-5-(+2))^2+(4-6)^2$$
$$=(-7)^2+(-2)^2,$$
$$=49+4=53,$$
$$\therefore\ AB=\sqrt{53}.$$

Now, the point that I have to make is this, that the formula in the form,

$$\overline{AB}^2=(-7)^2+(-2)^2$$

means nothing, though it leads to a true formula,

$$AB^2=(7)^2+(2)^2,$$

which carries out the theorem that the square on the hypotenuse equals the sum of the squares on the other two sides; whereas $(-7)^2$ cannot be said to represent the square on a side, for $-7\times-7=(-7)^2$ conveys no such meaning as the square on a side. Hence, although the general formula, *as it stands*, does not seem to carry out the principle of the square on the hypotenuse, &c., for *any* values of the co-ordinates, yet for any such values at pleasure, it can be at once transformed into a formula which is an analytical expression of this theorem, so that the formula is said to be general.

We note, further, that on extracting the square root, in the example above, according to the laws of signs, that we get two solutions,

$$AB=\pm\sqrt{53};$$

but the minus result must be discarded, since AB was tacitly assumed as positive from the beginning. This illustrates another point noted above.

THE CONDITION IN WHICH CARBON EXISTS IN STEEL.

From "The Engineer."

An inquiry into the condition in which carbon is present in steel as it is left by the cold-rolling—as well as in its hardened, annealed and intermediate states—has been made by Sir F. Abel and Mr. Deering for the Committee on Steel of the Institution of Mechanical Engineers. Two series of experiments were made, the earlier ones showing difference in the behavior of the hardened steel as compared with the cold-rolled and annealed steel, and in the amount of carbide of iron left by the oxidizing solution; and the second series were devoted to ascertaining the limits of strength of the chromic solution, within which the same percentage of carbide of iron would be obtained.

First Series.—Discs of steel, weighing about 6.5 grammes, were employed, about 2.5 in. diameter and 0.01 in. thick. Twelve were used, all of which were cut from the same strip of metal, the odd-numbered discs—1, 3, 5, etc.—being cut from one side of the axis of the strip, and the others—2, 4, 6, etc.—from the other side. The discs 1, 4, 7 and 10 were as received for the cold-rolling; Nos. 2, 5, 8 and 11 were annealed; and Nos 3, 6, 9 and 12 were hardened. The discs to be hardened were placed between two cast iron blocks, one being recessed to receive the plates, the other being quite flat. These blocks were equally heated to a bright red, a disc was then placed between them and allowed to remain till thoroughly heated; it was then instantaneously removed, and as quickly as possible caught and pressed between two cast iron surface plates. The discs to be annealed were bolted between wrought iron plates, $\frac{3}{8}$ in. thick, and so enclosed in a thin sheet iron box, 5 in. square and 2 in. deep. This was enclosed in a cast iron box about 15 in. by 6 in., and the intervening space filled up with flue dust—

thoroughly burnt soot—and the whole was then raised in an annealing furnace to a bright red heat, sufficient to scale but not peal the iron of the box. The fire was then slackened off, banked up with ashes, and the box left undisturbed in the furnace for twenty-four hours. From certain estimations made, the steel discs in contact with the wrought iron plates appear to lose carbon during the annealing. Of the annealed discs Nos. 2 and 11 were those which were in contact with the wrought iron plates. Disc No 6 was used for estimation of silicon, which was found to amount to 0.2 per cent. Total carbon was estimated in one disc of each kind, but an inside disc of the annealed series was examined to compare with those which had been annealed in immediate contact with the wrought iron. Total carbon was, as usual, estimated by decomposing the metal with cupric chloride containing sodium chloride. The filtration was conducted in the combustion tube itself, so that no loss could ensue from transferring the filtering bed and the carbonaceous matter. The discs were in all cases rubbed with fine emery and cleaned with ether before being used. The total carbon was found to be:

	per cent. carbon.
Disc No. 1 (cold-rolled).....gave	1.108
" 3 (hardened)..... "	1.128
" 5 (annealed, inside disc).. "	0.924
" 11 (annealed, outside disc). "	0.860

An estimation of the so-called uncombined carbon in three of the discs was made by generally heating them with hydrochloric acid of specific gravity 1.10. The annealed and cold-rolled discs dissolved much more rapidly than the hardened disc, the cold rolled disc furnishing the largest amount of dark-colored residue. The residue, collected on asbestos in combustion tubes, washed successively with water, alcohol, ether and water, were dried, and the carbon estimated by combustion:

	per cent. carbon.
Disc No. 7 (cold-rolled).....gave	0.096
" (annealed, inside disc).. "	0.052
" (hardened)..... "	0.935

Of the remaining four discs, three were submitted to the action of an oxidizing solution—potassium bichromate with sulphuric acid—made by adding to cold concentrated solution of bichromate one-twentieth of its volume of concentrated sulphuric acid. The discs were placed on sieves of platinum gauze in the center of 500 cubic cm. of the liquid with the following result: No. 4 disc (cold-rolled) solution of metal began at once with rise of temperature, and a very slight evolution of gas. Black particles in small quantity remained at the end of five days on the sieve, they were attracted by the magnet and appeared spangly under the microscope. No. 2 disc—annealed—solution did not commence till after the lapse of five hours; afterwards solution proceeded slowly; scaly residue left on the sieve resembled the above. No. 12 disc—hardened—metal at once attacked with considerable evolution of gas. At the end of five days a little buff-colored matter remained on the sieve, as well as spangles; the light-colored matter was probably silica. The residues which remained on the sieve were placed in the liquid for a further thirteen days. Finally collected and washed, dried, and burnt in oxygen as usual; the iron estimated after the experiment.

The following quantities, calculated on 100 parts of the discs, were found:

	Carbon. Per cent.	Iron. Per cent.
No. 4 (cold-rolled).....	1.039	5.87
No. 2 (annealed, out-side disc)	0.830	4.74
No. 12 (hardened).....	0.178	0.70

It will be seen that very nearly the whole of the carbon for the cold-rolled disc is left in the form of a carbon-iron compound; and from the annealed disc still more nearly the whole of the carbon. Thus:

	Total carbon. Per cent.	Carbon in residue from chromic treatment. Per cent.
No. 1 disc (cold-rolled).....1.108		—
No. 4 " "	—	1.039
No. 11 (annealed, outside disc)	0.860	—
No. 2 " " "	—	0.830

On the other hand, only about one-sixth of the total carbon of the hardened disc was left in the solid residue of the chromic treatment. In the latter case, too, the ratio of carbon to iron in the residue was greater than in the residue for the other two discs, thus:

	Carbon.	Iron.
No. 4 (cold-rolled).....	1	5.64
No. 2 (annealed).....	1	5.72
No. 12 (hardened).....	1	3.93

It is interesting to observe that in the case of the annealed and cold-rolled discs the ratios correspond very closely; they also correspond closely with the proportion of the elements of the iron carbide having the formula $Fe_6 C_1$. The last disc was used to see whether the iron carbide would resist the action of a chromic acid solution containing a large excess of sulphuric acid. There was left 0.84 per cent. of carbon and 1.104 per cent. of iron per 100 of disc. With this large excess of acid the carbide broke down.

Second Series.—The strength of the solution of chromic acid and the amount of sulphuric mixed with it was varied, and the different kinds are described as Preparations 1, 2, 3 and 4. From these points it was desired to ascertain whether its composition is independent, with rather wide limits, of the strength of the chromic solution employed; whether, within these limits, a constant quantity of carbide is obtained per 100 of steel; and how much of the carbon of this carbide, upon treatment with hot hydrochloric acid, would remain unconverted into hydrocarbons. The chromic acid solution used may be conveniently referred to that used for Preparation 2, containing 99 grammes of salt per 1,000 cub. cent. solution, sulphuric acid being added in the proportion of 0.9 gramme of acid to 1 gramme of the bichromate. The solution used in obtaining Preparation 1 was a little weaker, being 0.8th strength of the solution of preparation 2. Preparation 3 was produced with a much weaker chromic solution, its strength being 0.44. For Preparation 4 a hot solution of bichromate was mixed with the requisite quality of sulphuric acid, and the strength aimed at was double that of Preparation 2. The mode of treatment with the chromic solution was in all instances alike, and the experiments were made at ordinary laboratory temperatures.

Preparation 1.—Four pieces of the steel—from 7 to 7.5 grammes each—were exposed in separate vessels to the chromic acid solution; 1,000 cub. cent. to each piece of steel. At the end of two days there only remained small quantities of a black grey powder which was worked off into the liquid and exposed for several days to the action of the chromic solution, and were then collected together and treated with some fresh chromic acid. In a similar manner other portions of steel were treated with Preparations 2, 3 and 4, which led to the following results:

	Preparation 1.	Preparation 2.	Preparation 3.	Preparation 4.
Carbide obtained per 100 of steel	13.25	14.16	153.4	4.66
Composition per 100 of carbide				where 400 is $Fe_2 C$.
Carbon	7.31	7.21	6.84	11.77
Iron	90.42	90.64	91.50	80.57
Water	2.37	2.27	1.63	5.57
Atomic ratio	$Fe_{2\cdot 85} C_1$	$Fe_{2\cdot 894} C_1$	$Fe_{2\cdot 867} C_1$	—
Parts of carbon obtained in form of carbide per 100 of steel	0.969	1.021	1.049	0.266
Parts of carbon unconverted into hydrocarbons by treatment of carbide with H Cl:				
Per 100 of carbide	{1.410 / 1.238}	1.269	0.836	
Per 100 of carbon in carbide	{20.87 / 16.93}	17.60	12.22	

An examination of the foregoing results suggests the following conclusions:

1. The two chromic solutions used for Preparations 1 and 2 gave very similar results both in respect of the percentage of product obtained from the steel and of the percentage composition of the product. The third, a much weaker solution, furnishes results which, allowance being made for the smaller quantities of substance dealt with and inherent analytical difficulties, must be regarded as

closely resembling those obtained with the other two solutions.

2. The results obtained with Preparation 4, the strongest chromic solution, indicate that the limit of concentration of the oxidizing solution which the separated carbide is capable of resisting has here been exceeded. Not only has there been in this case a very considerable loss of carbon as hydrocarbons—or possibly as a soluble product of oxidation—but the iron in the separated carbide has also been to a considerable extent attacked, and only a relatively small proportion of the carbide remains, together with separated carbon, the latter partly in a hydrated form, and possibly also in some partially oxidized insoluble form.

3. The small amounts of water obtained by the combustion of Preparations 1, 2 and 3 may indicate that, in these also, small quantities of carbohydrate are present with the iron carbide. This may result from the action of the chromic solutions on the carbide first separated, and may account for the not very definite, though on the whole uniform, atomic ratio of iron to carbon in the products of Preparations 1, 2 and 3.

4. If the carbon, unconverted into hydrocarbons by treatment of the products with hydrochloric acid, be deducted from the percentage of total carbon in the products of Preparations 1, 2 and 3, the results exhibit a uniformity which, if accidental, is somewhat remarkable. Thus:

	Preparation 1.	Preparation 2.	Preparation 3.
Percentage of total carbon.....	7.31	7.21	6.84
Less carbon unconverted into hydrocarbons.....	1.38	1.27	0.84
There remains of carbon per cent.	5.93	5.94	6.00

The atomic ratio of this residual percentage of carbon to iron is as 1 to 3.270 of iron.

5. The carbon separated in the solid form as carbide and carbohydrate more nearly approaches the total amount (1.144 per cent.) of carbon contained in the steel in the case of No. 3, when the weakest chromic solution was employed—a result which was anticipated.

These results serve to confirm the view that the carbon of cold-rolled steel exists, not simply diffused mechanically through the mass of the steel, but in the form of an iron carbide, a definite product capable of resisting the oxidizing effect of an agent which exerts a rapid solvent action upon the iron through which the carbide is distributed. It is to be hoped that opportunity may be found to continue these experiments with unfused cementation steel, and with other ingot steels in the same and in different conditions of temper, using the weakest chromic solution which gave the most favorable results.

MOTION ON THE SURFACE OF A SPHEROID.

By J. E. HENDRICKS.

Contributed to Van Nostrand's Engineering Magazine.

As several articles have been published in this Magazine during the past year, in which the deflecting force of the surface of a revolving spheroid, as the earth, for instance, has been considered, and as the results obtained by the respective writers differ, and mine, at least, is defective and incorrect, I beg leave to substitute for my papers, published in the October and January Nos., the following proposition and discussion:

Proposition:—"When a body rotates about an axis, it is in consequence of this rotation, simultaneously rotating about any other axis which passes through the same point, with an angle of rotation which is represented by the projection upon this new axis of the line which represents the original angle of rotation." (See Peirce's Analytical Mechanics, Sect. 25).

Let P represent the point where the new axis pierces the surface of the spheroid, and let λ denote the latitude of P; then, by the above proposition, the tangent plane will rotate about P with an

angular velocity $= V \div R \sin \lambda$. Hence a body moving over the point P, in any direction, with a velocity v, will, by the rotation of the tangent plane, be constrained to describe in space the spiral of Archimedes, whose equation is $u = a\,\theta$; and, when $\theta = 2\pi$, $u = v$ multiplied by the time occupied by the tangent plane in making one complete rotation about P, $= 24\,v \div \sin \lambda$; therefore $a = 24\,v \div 2\,\pi \sin \lambda$, and $\frac{1}{2}a = 6v \div \pi \sin \lambda$.

Now the deflecting force at P is represented by the centrifugal force due the velocity v at the origin of the spiral, which is equal to v^2 divided by the radius of curvature at P; but the radius of curvature at the origin of the spiral of Archimedes is known to be $\frac{1}{2}a$, hence

$$f = v^2 \div \tfrac{1}{2}a = \tfrac{1}{6}\,v\,\pi \sin \lambda.$$

Because $V = \frac{1}{12}\pi R$, and $V^2 \div R = \frac{1}{289}\,mg$,

$$\therefore f : \tfrac{1}{289}mg :: \tfrac{1}{6}\,v\pi \sin \lambda : V^2 \div R,$$

whence, substituting for V^2, we get

$$f = \frac{\frac{1}{289}mg24\,v \sin \lambda}{\pi R}.$$

No account is taken here of the centrifugal force, resulting from the relative motion of the body about the earth's polar axis, as was done in my former papers, for the reason that this force affects only the position of the origin of the spiral, and not at all the elements of the spiral, and, therefore, not at all the deflecting force at P.

If we suppose the velocity v to be 60 ft. per second, which is equivalent to about 40 miles per hour, and for $\lambda = 45°$ and R = 3956 miles, we get

$$f = \tfrac{1}{5183}\,mg,$$

that is, the lateral pressure will be $\frac{1}{5183}$ of the weight of the moving body.

This result agrees with that obtained by Mr. Ferrell in the *Math. Monthly.*, Vol. II., p. 380, but is obtained by a more simple analysis than that employed by Mr. Ferrell.

FIRE PROTECTION IN THE DRY GOODS DISTRICT.

By FRANCIS B. STEVENS.

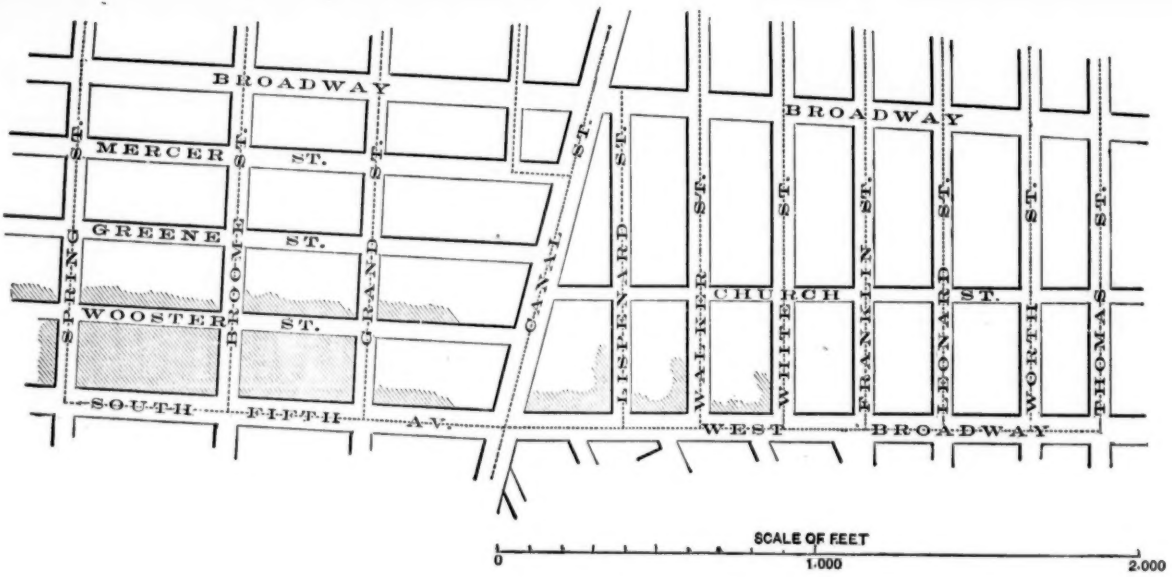
Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

THE *Herald* has thoroughly shown the great danger threatening the City of New York from what has aptly been termed "the coming conflagration." Notably, its editorial of August 28th sums up the risk, and the consequences of a great conflagration in the dry goods district

The list of uncontrollable fires in cities is a long one, but is so, for the very obvious reason that improvements in the methods of extinguishing fires have never kept pace with increased difficulties and dangers to be met.

By the great fire in London in 1666, the number of dwelling-houses destroyed, exclusive of other buildings, was 13,200. These houses were low, built generally of wood, or of wooden timbers filled in with brick; and the only means then relied on to extinguish fires were water buckets passed from hand to hand. This fire led, almost immediately afterwards, to the introduction of fire engines and leather hose from Holland.

By the great fire in New York in 1835, six hundred and seventy-four buildings were destroyed. This fire occurred in intensely cold weather, accompanied by a high wind, and the means relied on to subdue it were a succession of hand fire engines extending from the river to the fire; one engine pumping into the other, and the last one delivering a feeble and intermitting stream. These engines did not produce the slightest effect on the fire, which, fanned by a furious northwest wind swept everything before it—broadening as it progressed—until it reached the East River. The destruction of some of the buildings by gunpower did not arrest the fire, but merely restrained it in a limited degree from spreading laterally. The causes assigned for the destruction of three hundred and two buildings in a portion of the same district—in fair weather in the summer of 1845, by a fire spread by an explosion—were the deficiency in the capacity of the water pipes, and the want of a sufficient number of proper hydrants.



THE DRY GOODS DISTRICT.

The dotted lines show the water pipes. The Mains are led from the river at West Street, through Canal Street to its intersection with West Broadway and South Fifth Avenue; and through these two streets to the distance shown.

The Branch Pipes run from these Mains through each of the cross streets. shown.

The Fire Department of New York, with its present efficiency, system, and appliances, could certainly have controlled and arrested the fires of 1832 and 1845; for the buildings were low and in great part altered from dwelling-houses; but the experience of the Boston fire shows that the means at present available in New York—although its fire department is probably the best in the world—would be insufficient to control with any certainty a great fire in the dry goods district, even with an independent supply of water from the Central Park Reservoir. The completion of the new aqueduct cannot alter the case materially, as far as this district is concerned, for the Reservoir now contains many times the quantity of water that could be used on any fire.

The danger threatening the dry goods district arises from the great height of the buildings, combined with the great quantity of combustible material stored in them up to this great height; and combined also with the great number of these buildings—occupying about one hundred acres—and their contiguity, front and rear.

It has been held, that it would be impossible to arrest a great conflagration in the dry goods district excepting by the use of explosives; but this cannot be taken for granted, on the contrary it may be that the opinion of engineers best qualified to judge, would be against relying on the use of explosives to isolate a fire by the destruction of a sufficient number of buildings; and that with the modern improvements and appliances, water would be the better means to rely on. Since the Chicago and Boston fires, a great advance has been made in fire protection, both by the improvements in the methods and appliances, and by the treatment of the subject by able engineers. Prominent among the improvements are those in the application and extension of the stationary system by which water under pressure is delivered directly on a fire, without the intervention of movable engines—a system that in its most simple form, the application of water led from a reservoir placed on an elevation, was in use by the Romans. The introduction of the steam pumping engine, and its many improvements, has made this system applicable to any locality, and has given the means of increasing both the volume and the pressure of the water used to any extent. The difficulty of making a complete application of this system arises from the fact, that a pressure sufficient for the purpose would be too great for the strength of the existing water mains and plumbing work, and also for general convenient use. But while this objection, on account of the expense involved, would preclude its general adoption in New York—excepting, perhaps, in the far future—it may be found not unwarrantably expensive to apply it to certain parts of the city, where the risk of an uncontrollable fire is greatest.

The following plan, based on the stationary system, is submitted in an endeavor to show that the risk of a great conflagration, either originating or spreading in the dry goods district, could be made as small as in any other part of the city, at a cost within the repaying limit.

The objects sought are, 1st: That in the case of an ordinary fire, there shall always be a certainty of having water in sufficient quantity and under sufficient pressure, ready for instantaneous use; and that the time elapsing between the alarm and the application of water shall be reduced to the lowest possible limit; so that the damage done by fire and water may both be diminished; 2d, that by the machinery kept always in reliable working order for effecting the first object, additional means be added; by which a great fire either originating in the district or threatening from without. may be controlled or repelled.

The map annexed shows the district. It occupies compactly the whole space shown, with the exception of a small portion on West Broadway and on South Fifth avenue, shown by shaded lines; and extends in length from the center of the block between Duane and Thomas streets to Spring street, a distance of about 3,350 feet; and in breadth supposing it to reach 300 feet east of Broadway, about 1,300 feet, containing including the streets about 100 acres. The highest point is at the intersection of Leonard street and Broadway, which on the street level is 35 feet above high-water mark; and the lowest point is at Canal street and West Broadway, and is 7 feet above high-water mark. The po-

sition of the water mains and branch pipes are shown by the dotted lines.

These mains and pipes are to form an independent system, connected to steam pumps, located on West street, at any available place within three or four hundred feet from Canal street. The water is to be taken directly from the river, whenever it may be necessary to do so; and a connection is also to be made with the Croton pipes in Washington street, that fresh water may be used if desirable, and if only a limited quantity is wanted. The maximum pressure at the pumps available in emergencies, is to be 173 lbs. per square inch, equivalent to a head of 400 feet of fresh water; and for ordinary use a lower pressure—about equal to that generally kept on the steamer engines—is to be always maintained in the pipes and at the hydrants, and subject to regulation there. This pressure is to be increased immediately at the pumps when necessary, up to the maximum pressure. The maximum quantity of water delivered, is to be 12,700 gallons a minute, equal to the quantity that would be delivered from forty-eight steamer engines; supposing each to deliver continuously, and without intermission, 265 gallons a minute. This maximum quantity of 12,700 gallons a minute is to be always available for use within four minutes after call; and the quantity kept available at any hydrant in the district for instantaneous use under a pressure of about 100 lbs. use is to be equal to that delivered by four steamers.

The water mains are to be 24 inches in diameter, duplicated, and to run in pairs from the pumps in West street to West Broadway; thence south through West Broadway to Thomas street, and north through South Fifth avenue to Spring street. The branch pipes are to be 20 inches in diameter, and are to run, duplicated in pairs, from these mains through each of the eleven cross streets shown on the map, to a distance of 300 feet east of Broadway; and at every intersection of two streets throughout the whole district, are to connect by pipes 8 inches in diameter with four hydrants—one being at or near the corner of each street. The branch pipes are also to connect with additional hydrants placed in the cross streets at a distance of 100 feet apart. All the hydrants are to have four nozzles for hose delivery.

By the means described, no spot in the district would be farther than 300 feet from 12 hydrants, capable of delivering the whole maximum quantity of 12,700 gallons a minute, and under a pressure—if necessary—of 173 lbs, a square inch. And as the greatest distance of any building from a hydrant in the eleven cross streets would not be over 50 feet—in the three longitudinal streets below Canal street would not be over 100 feet, and in the five longitudinal streets above Canal street would not be over 220 feet; the interval of time elapsing between an alarm and the certain application of water, could be reduced to the lowest limit, supposing the management to be equal in discipline and efficiency to that now maintained by the Fire Department.

But these means—powerful as they are—would not be sufficient to depend on in case of a great fire in the district, originating either within it or outside of it, if spread by a combination of accidents, and fanned by a high wind.

In such a case, water could not be thrown from the street level to the necessary distance from the leeward; and on account of the narrowness of the streets and the height of the buildings, it could not be directed to cover any given point off the direct line of the streets.

The means proposed to control such a conflagration are, that at or near the intersection of every two streets, two wrought iron stand pipes, each 8 inches in diameter, are to be placed, and are to run upwards on the front of one of each of two corner buildings to its top. These pipes, similar in construction and position, but larger than those now common for fire protection on the fronts of many buildings, are each to connect with one of the 20-inch branch pipes, and each are to terminate in a play stationary pipe, arranged to be pointed in any direction, having a nozzle three inches and three-eighths in diameter, and estimated to throw, from an elevation eighty feet above the street level, with an available head of two hundred and twenty-eight feet, 3,175 gallons of water a minute, to a horizontal distance—in calm weather—of four hundred and four feet. The especial use of such heavy streams would be to arrest from the leeward, either directly or obliquely,

a great fire urged onward by a high wind, and although the horizontal distance reached against a high wind would be very much less than four hundred and four feet, it is submitted that the streams from the elevation of their delivery, from their pressure, and from their size and weight, would be sufficient to arrest any fire. No spot in the district would be distant over 270 feet from four nozzles, throwing unitedly 12,700 gallons a minute, and the district, by the plan described, would be fenced around its whole circumference by stand pipes arranged that the streams from four of them would be available to repel at any point, a fire originating outside of the district. Of course, such streams should not be thrown excepting in a case of extreme necessity, smaller ones could be used when sufficient.

The management of using the water should be placed absolutely under the control of the Chief of the Fire Department, that the stationary system might not work in conflict with the present one.

ESTIMATED COST.

Pipes and hydrants	$360,000
Engine house, engines, pumps and boilers	140,000
Eighty stand pipes and attachments	60,000
Contingencies	60,000
	$620,000

MAINTENANCE.

Yearly cost of attendance, fuel and repairs	$75,000

A fair time for executing the work would be five months. The steam pumps and pipes are of merchantable sizes, and the boilers could be made within three months.

The 24-inch mains and the 20-inch branch pipes are of sufficient diameter to allow the plan to be extended by continuing the mains from Thomas street, through West Broadway, Church, Morris and Whitehall streets, to the foot of the latter street; and by running branch pipes from these mains to the eastward of Broadway and to the westward of West Broadway.

The plan proposed may be criticised for overdoing the matter, for having available for use more water and greater pressure than necessary, for the large size of the pipes, for the nearness of the hydrants, and for the use of streams of the exaggerated size of three inches and three-eighths in diameter from elevated stand pipes. The answer is, that the case to be provided for is exceptional, both as to the amount involved and to the impending danger, and history of great fires teaches, that the means employed should be in excess rather than deficient.

Any plan that would accomplish the object, must be far in advance of existing methods, and as it would be applicable only to a particular districts where extraordinary precautionary measures should be taken, it is unlikely that it could be carried into effect unless by an organization of the owners of the district to be protected, aided by the insurance and acting under legislative authority.

MEMORANDA OF DETAIL AND OF ESTIMATES

ENGINE HOUSE.

The engine house is to be placed on any available lot about 25 feet front, and 100 feet deep, on West street near Canal street. The building to be of brick, plain, two stories in height, and without a basement or partition.

The lower story is to be on the street level.

SUCTION PIPES.

From a well sunk in the engine house to a depth of 13 feet below high water mark, two suction pipes, each 24 inches in diameter, are to run side by side to the face of the bulkhead, a distance of 200 feet, and are to be continued at the same level, suspended under the pier selected, nearly to its end, a distance of about four or five hundred feet, for the purpose of obtaining clear water, and are to terminate in a strainer capable of being raised for examination. Two suction pipes, each 10 inches in diameter, and each with water gates, are also to lead from the rear of the engine house and to connect with the Croton main in Washington street.

DELIVERY PIPES.

The 24-inch delivery mains are to be 1.04 inches thick, and the 20 branch pipes are to be 0.92 in. thick. The pair of 20-in. branch pipes, running through each of the cross streets are to be connected at their extremities—300 feet east of Broadway—by a semicircular bend pipe 10 inches in diameter, that the water may be

forced through one pipe of the pair of mains, and returned through the other pipe; so that a circulation may be maintained in winter or when necessary. In each of these connecting pipes a 10-inch water gate is to be placed, so that one pipe of the pair throughout the whole system may be always available for use, in case the water should be withdrawn from the other pipe for repairs, or to clean or to paint the interior surface.

No other water gates than the 10-inch ones mentioned are to be placed anywhere on the lines of mains and branch pipes.

The mains are to be emptied and the suction pipes are to be flushed, by connecting in the engine house, each 24-inch main and suction pipe by a 10-inch pipe having a 10-inch water gate.

HYDRANTS.

The hydrants, 300 in number, are to be connected alternately to one of each of the pair of 20 branch pipes by an eight-inch supply pipe. Each hydrant is to have a main water gate at its bottom, and an independent water gate at each of the four nozzles for hose delivery.

STEAM PUMPS AND ATTACHMENTS.

To be eight direct-acting horizontal steam pumps, such as are now sold here by at least four reliable makers.

Each pump to be capable of delivering 1,588 gallons of water, under a head of 400 feet. These engines are to be placed on the first floor of the engine house at about the street level. The eight pumps are to be divided into sets of four pumps each, and each pump, in each one of the two sets, is to be connected in the engine house to one of each of the pair of 24-inch suction pipes by a 12-inch pipe having a 12-inch water gate, and to one of each of the pair of 24 delivery pipes by a 10 pipe having a 10-inch water gate. A relief valve with an area of 200 square inches and loaded to 173 pounds to the square inch, is to be placed on each of the 24-inch delivery pipes in the engine house, and each is to be connected to a 12-inch pipe discharging into the river at the face of the bulkhead, to prevent over-pressure in the pipes. Also, when a circulation is to be maintained in the pipes, one of these valves is to be raised by a hand attachment to allow for the discharge.

BOILERS.

To be eight locomotive boilers, of the consolidation pattern, each 25 feet long, and 54 inches in diameter, and each to have its grate surface enlarged to measure 36 square feet. These boilers are to be placed on the second floor of the engine house. The fires are to be urged by the contraction of the blast to the same extent as in locomotives. Steam is to be held on all of the boilers day and night, by means of the fire kept in the furnace of one; the fire boxes of the other seven being filled with kiln—dried wood ready for instantaneous ignition; the kindling being aided by a fan blast. The net horse power required to deliver 12,700 gallons of water a minute, under a pressure of 173 lbs. per square inch, is 1280; and these boilers would be ample for that service. Fresh water should be used in the boilers.

ESTIMATE OF AVAILABLE HEAD.

The pressure at the pumps being maintained at 173 lbs. per square inch, equivalent to a head of 400 feet of fresh water, the head on the street level at the highest point of the district, viz., at the intersection of Broadway and Leonard street would be 31 feet less, or 369 feet; and the head at the top of the eight-inch stand pipes, supposing the water at rest and no losses to occur by its movement, would be 369 feet, less the height of the stand pipe—80 feet—equal to 289 feet.

The losses of head resulting from the motion of the water in delivering 12,700 gallons a minute through the mains, branch pipes and four stand pipes, would be as follows:

	Ft.
Loss by friction in the two 24-inch mains, each 4,000 feet long	16.20
Loss by friction in the two 20-inch branch pipes, each 1,000 feet long	10.10
Loss by friction in the four 8-inch stand pipes, each 110 feet long	27.10
Loss at the intersections of the 24 and 20-inch pipes	0.65
Loss at the intersections of the 20 and 8-inch pipes	6.35
Loss at the 8-inch bends	0.60
Total loss	61.00

Deducting this from the static head of 289 feet, there remains 228 feet available

head at four nozzles—each three and three-eighths inches diameter—in the highest part of the district, estimated to throw the water a distance of four hundred and four feet.

THE USE OF FRESH WATER.

The damage done by salt water over that done by fresh water, in extinguishing a fire, cannot be very great.

Salt water was exclusively used in New York up to the year 1842; and it is believed that the introduction of fresh water made no marked change in the damage by water. But if it should be deemed advisable to use fresh water for an ordinary fire, the Croton main in Washington street would be sufficient for that purpose. In the case of a great fire the difference in the damage, by salt over fresh water, would be too small for computation in comparison with the loss of property. To bring the maximum quantity of 12,700 gallons a minute from the Central Park Reservoir would require an independent main 36 inches in diameter, costing over $300,000, without allowance for excavation in rock. A smaller pipe would not answer, for the whole head of the Reservoir above high water mark of 119 feet would be absorbed by the frictional resistance of 12,700 gallons of water a minute, led in a pipe 30 and 3-4ths inches in diameter, and 24,500 feet—the estimated distance—long.

LOCATION OF ENGINE HOUSE.

If no available lot for the engine house could be had near Canal street, any lot on West street, between Duane and Spring street, would answer; and the mains could be led thence directly to West Broadway.

Detailed Estimated Cost.

THE 24-INCH PIPES.

Under the pier, 500 feet long; in Canal Street, 2,600 feet long; in West Broadway, 3,400 feet long; and this duplicated is 13,000 feet for the pair, which, at $5.70 a foot, including manholes and outlets, and at $1.80 a foot for laying, is	$97,500
Additional contingent cost for laying the suction pipes across West Street	13,500
Strainers and attachments at the ends of the suction pipes	2,000
Total	$113,000

THE 20-INCH PIPES.

The total length in the eleven cross streets is 15,000 feet, which duplicated, and taken at $4.50 a foot for pipes and $1.50 feet for laying, is	$180,000
Cost of mains and branch pipes	$293,000

THE OTHER PIPES.

Two 12-inch relief pipes, with relief valves	4,000
Two 10-inch pipes connecting with the Croton	1,500
In the engine house, eight 12-inch suction pipes, eight 10-inch delivery pipes, and two 10-inch pipes connecting suction and delivery for flushing	3,500
Connecting the branch pipes and hydrants, three hundred 8-inch pipes, each 30 feet long	6,000
Laying and connecting them	5,000
Cost of all water pipes	$313,000

WATER GATES.

In the engine house, ten 12-inch and fourteen 10-inch water gates; and at the terminations of the branch pipes, eleven 10-inch water gates	$3,000

HYDRANTS.

Three hundred hydrants	44,000
Cost of pipes and hydrants	$360,000

ENGINE HOUSE, STEAM PUMPS AND BOILERS.

Land, 25 feet by 100 feet	$25,000
Brick building	38,000
Eight horizontal direct-acting steam pumps, each to deliver 1,588 gallons a minute, at $2,500 each	20,000
Connecting, and cost of steam pipes, feed pumps, valves, gauges, and tools	18,500
Eight locomotive boilers, consolidation pattern	25,000
Main chimney, eight feet in diameter and 80 feet high, and eight connecting chimneys, all of No. 8 iron	3,500
Boiler attachments	10,000
Total	$140,000

STAND PIPES.

Eighty 8-inch stand pipes, 85 feet long made of steam pipe, at one dollar and thirteen cents a foot net, costing each	$96
Erecting each	54
Play-pipes, with nozzles three and three eighths inches in diameter, and attachments for direction, costing each	150
Connecting each stand-pipe and the 20-inch branch pipe	100
Two water gates for each pipe, one below the surface of the ground, and the other at the nozzle, at $50.00 each	100
Cost of each pipe and nozzle	$500

Add to each pipe for contingencies....	$250
Cost of the eighty pipes erected complete, with nozzles.....	$60,000

YEARLY EXPENSE FOR MAINTENANCE.

Pay roll of 15 men stationed night and day in the engine house	$20,000
Coal to keep steam constantly on the boilers, night and day, at three tons of anthracite a day.....	6,000
Fuel for extinguishing fires, estimated at the maximum at 200 tons of Cannel coal and 500 cords of pine wood.	5,000
Repairs.....	10,000
Contingencies.....	34,000
Total.....	$75,000

RECAPITULATION.

Pipes and hydrants.....	$360,000
Engine house and pumps.....	140,000
Stand pipes.....	60,000
Contingencies.....	60,000
	$620,000
Maintenance per annum.....	$75,000

ON THE PHYSICAL CONDITION OF IRON AND STEEL.

By PROF. D. E. HUGHES, F. R. S.

A Paper read before the Institution of Mechanical Engineers.

In a paper read before the Royal Society, May 5th, 1879, entitled "On an Induction Currents Balance, and experimental researches made therewith," the author showed that this instrument was extremely sensitive to all molecular changes in metallic bodies. Finding that its powers were remarkably suitable for researches upon the molecular change which takes place in iron and steel when tempered, he made with it a series of researches to determine the cause of tempering in steel. The results of these the author laid before the Institution of Mechanical Engineers (Proc. 1883, p. 72) in a paper "On the Molecular Rigidity of Tempered Steel." In this paper the author advanced the theory that the molecules of soft iron were comparatively free as regards motion amongst themselves, whilst in hard iron or steel they were extremely rigid in their relative positions.

The author has since widened the field of research so as to embrace all the physical changes which occur in iron and steel through chemical alloys, mechanical compression or other strains, annealing, and tempering. The results of these researches he now embodies in the present paper. Believing it necessary that we should be able to tell the physical state of any piece of iron, without destroying or changing that state, the author has sought for and tried several methods, which gave any hope of success in this direction. The physical state of iron has a marked influence upon its electrical conductivity. The differences thus indicated, however, are not wide enough to be appreciated except with metal in the form of wire; and in order to perceive small changes, such as small differences of temper, we should require a wire at least 250 yards in length. The author has found, however, that by the application of certain phenomena belonging to magnetism we are enabled to perceive clearly the slightest change in the molecular structure of iron or steel, through all degrees of annealing to the finest differences in tempering, and this with pieces of any form or dimensions.

It is already known that soft iron will take a higher degree of magnetism, and retain it less, than steel; and that tempered steel retains magnetism more than soft steel. Consequently we might expect, that by the aid of an instrument which could give correct measurement of degrees of magnetism, we should be able to include all varieties of iron and steel, between the two extremes of softness as in annealed iron, and hardness as in highly tempered cast steel. The author soon found that this was not the case when pieces of iron were magnetized to saturation, or even partially so.

In a recent paper upon the theory of magnetism* the author said: "During these researches I have remarked a peculiar property of magnetism, viz., that not only can the molecules be rotated through any degree of arc to its maximum, or saturation, but that, whilst it requires a comparatively strong force to overcome

* Society of Telegraph Engineers, May 24th, 1883.

its rigidity or resistance to rotation, it has a small field of its own through which it can move with excessive freedom, trembling, vibrating, or rotating through small arcs with infinitely less force than would be required to rotate it permanently on either side. This property is so marked and general that we can observe it without any special iron or apparatus."

The author has found, by employing extremely feeble magnetizing powers, such as a weak current of electricity only just sufficient for measurement (or the current from one Daniell cell reduced, as found best for the dimensions of the iron, by passing it through resistance-coils varying from 10 to 100 ohms), that the following laws hold with every variety of iron and steel:—

1. The magnetic capacity is directly proportional to the softness or molecular freedom.
2. The resistance to a feeble external magnetizing force is directly as the hardness, or molecular rigidity.

The author has proved this to be the case with sixty different varieties of iron and steel furnished direct from the manufacturers. And he has found that each variety of iron or steel has fixed points, beyond which annealing cannot soften, nor tempering harden; consequently, if all varieties were equally and perfectly annealed, each variety would have its own magnetic capacity, or its specific degree of value, by means of which we could at once determine its place and quality.

If in place of several varieties we take a single specimen, say hard-drawn Swedish iron wire, and note its magnetic capacity, we find that its value rises rapidly with each partial annealing, until an ultimate softness is obtained, being the limit of its molecular freedom. We are thus enabled to study the best methods of annealing, and to find at once the degree of softness in an unknown specimen.

Similarly, when we temper annealed iron and steel, we find that we can follow out each degree of temper up to ultimate molecular rigidity; and we may thus appreciate in an unknown specimen of unknown temper the degree of its hardness.

We have thus in each piece of iron or steel a limit of softness and hardness. In soft Swedish iron, tempering hardens but 25 per cent. on the scale adopted, whilst mechanical compression, such as hammering, hardens it 50 per cent. In cast steel, tempering hardens it 400 per cent., whilst mechanical compression gives but 50 per cent. Between cast steel and Swedish iron we find a long series of mild steel, hard iron, &c., varying in their proportionate degree between the two extremes just mentioned.

The theory which the author has advanced, of molecular freedom as in soft iron, and molecular rigidity as in cast steel, fully explains all the changes which we are enabled to perceive and measure; but it is not absolutely necessary to accept the theory in order to appreciate the results. For, leaving theoretical considerations aside, we have one proved fact, viz., that the magnetic power or capacity of a piece of iron, under the influence of an external limited magnetizing power, depends upon its softness; and that the retention of magnetism, when the external power is withdrawn, depends upon its hardness. The same degree of temper or annealing, upon the same iron or steel, gives invariably the same readings; but the slightest change—say from a straw-colored temper to a blue—gives very wide differences.

Description of Apparatus.

The instrument which the author has constructed and used in these experiments, and which he has named a "Magnetic Balance," consists of a delicate magnetic needle, suspended by a silk fibre; it is 5 centimetres in length, and its pointer rests near an index having a single fine black mark for its zero. The movement of the needle on either side of zero is limited to 5 millimetres by means of ivory stops or projections. When the north end of the needle and its zero index are north, the needle rests parallel with its index; but the slightest external influence, such as a piece of iron 1 millimetre in diameter placed at 10 centimetres distance deflects the needle to the right or left, according to the polarity of its magnetism, and with a force proportionate to its magnetic power. If we place on the opposite side of the needle, and at the same distance, a wire possess-

ing absolutely the same polarity, of similar name and force, the two balance each other and the needle returns to zero; and if we know the magnetic value required to balance the first piece of iron we know the magnetic value of both.

The iron, B (which may be in the form of wires, rods, bars, plates, or any shape or size desired),* is placed at a fixed distance (preferably 10 or more centimeters) resting against a fixed brass stop. The centre of the iron should be in a line with the centre of the needle, and it should be placed at right angles to the needle, lying horizontally east and west, so as to be free from the directing influence of the earth's magnetism.

The compensator, placed upon the opposite side of the needle, and at a distance of 30 centimetres, consists of a powerful steel bar-magnet, 3 centimetres width, 1 centimetre thick, and 6 centimetres long. This turns upon its axis, carrying with it the pointer to indicate its degree of angular displacement on the graduated circle. Generally this bar-magnet is parallel with the needle, the pointer of the compensator and the needle being at zero; but when we wish to measure the amount of magnetism in the piece of iron the bar-magnet is made to pass through an angular displacement necessary to balance this force, and its index readings on the graduated circle are taken as the comparative values.

In order to magnetize the iron by an electrical current, a coil of insulated copper wire is placed near the needle, the iron then becoming the core of an electro-magnet.

Now as this coil, independently of its iron, acts upon the needle, this action must be balanced by an opposing coil, G, on the opposite side. The position and power of these two coils can be adjusted by means of the lever H, which allows us to find a position where the two coils completely neutralize each other. If we introduce iron in the coils on either side, the balance is destroyed, and we have solely the magnetic influence of the iron core, whose value we find by an equal opposing magnetism brought into play by the rotating magnetic compensator.

A reversing key serves to change the direction of the current, and thus any difference between north and south polarity in the iron core can be observed. One Daniell cell is all that is required as a battery; but great care must be taken that its electromotive force is a constant, otherwise all variations in the battery would be read as variations in the quality of the iron itself; and we need in addition a series of resistance coils from 10 to 100 ohms, in order to reduce the current sufficiently to bring the whole series, from soft Swedish iron to cast steel, into range. Separate and finer determination can then be separately made by an extremely weak force for soft iron, and full or increased battery power for tempered steel. A series of different sized coils is necessary, whenever we vary greatly the diameter of the core. The first size, with an internal core-opening of one centimetre, will test bars and rods of wire, from one centimetre diameter to the finest needle; but for larger bars, plates, &c., coils must be used which allow free passage for the iron into the core. Great care and some practice is necessary in the use of the instrument, so as to ensure that the iron is placed in a neutral field; but when we have really obtained the necessary conditions we can take several readings in a single minute, with an invariable result for the same kind of iron.

All irons and steel have some traces of remaining magnetism; it is therefore necessary that a double reading (north and south) should be taken by means of reversed currents. In this case the quadrant is divided into 360° on each side of zero; and the total value of north and south polarity added together is that given in the following tables of magnetic capacity.

Several methods of observation can be employed with the magnetic balance, the usual one being that already described; but there are many others, such as magnetizing all specimens to the same value and noting the amount of current required. We may also observe the remaining magnetism after the cessation of the current; the influence of a weak current after the passage of a strong, &c. Many of these methods give interesting facts, particularly useful to those making researches upon the cause of magnetism.*

* The smallest rods yet tested have been fine sewing needles, and the largest bars of 5 centimeters diameter, 1 metre long.

* The author has not patented this instrument, giving it freely to the scientific and manufacturing world.

By means of this instrument the author has tested sixty brands of iron and steel, mostly in the form of wires. A wire one millimetre diameter and ten centimetres long was the standard size used, as we can more readily temper small wires than large rods. In all comparative experiments between iron of different grades, we must have one standard form to which all the rest must be similar in form and size. Thus we could not compare a square or flat bar with a piece of wire; but if all pieces have the same form, then any difference observed between them must be due to their comparative softness, from which we can deduce the quality and place of each on the line ranging from soft iron to cast steel.

Influence of Annealing upon the Molecular Structure of Iron and Steel.

The magnetic balance shows that annealing not only produces softness in iron, and consequent molecular freedom, but it entirely frees it from all strains previously introduced by drawing or hammering. Thus a bar of iron drawn or hammered has a peculiar structure, say a fibrous one, which gives a greater mechanical strength in one direction than another. This bar, if thoroughly annealed at high temperatures becomes homogeneous in all directions, and has no longer even traces of its previous strains, provided that there has been no actual mechanical separation into a distinct series of fibres.

Table I.

INFLUENCE OF ANNEALING UPON SWEDISH IRON, SAMPLE G.

	Approximate Temperature.		Degrees of softness indicated upon the Magnetic Balance.
	Cent.	Fahr.	
Wire hard-drawn as furnished by maker....	—	—	230°
Annealed at black heat.....	500°	950°	255°
Annealed at dull red.	700°	1300°	329°
Annealed at bright red	1000°	1800°	438°
Annealed at bright yellow.....	1100°	2000°	507°
Annealed at bright yellow white.....	1300°	2300°	525°

From Table I. we see that a regular increase of softness occurs as the temperature at which Swedish iron is annealed increases, the maximum being at a point under that of fusion.

Some difficulty was experienced in annealing all wires to the same standard. The method employed at first was to place the wires in an iron tube heated to the desired temperature; but the temperature of the tube was extremely variable, and also it was found that an interchange of carbon takes place between the tube and wires. Steel wires rapidly lose their carbon, and thus become softer at each successive annealing; whilst the purest iron absorbs carbon, until it contains exactly the same proportion as the tube itself. It is well known that iron wires at red heat, placed in a porcelain tube through which a current of carbureted hydrogen is passing, will absorb sufficient carbon to become hard steel.

Experiments regarding the time required for perfect annealing showed that whilst hard steel required several hours, soft iron might be cooled in a few minutes without losing its degree of softness; consequently, knowing the great value of high temperature, the author adopted the following method. The tube was heated to a white heat or otherwise, the iron wires to be annealed were introduced quickly, and the instant they had the same temperature, they were withdrawn and simply allowed to cool in the air. The wire employed being 1 millimetre diameter, the whole operation was complete in two minutes. This is not suggested as the best practical method of annealing, although in the case of these wires it produced the best result; but the experiments show that, whatever method is employed, the heating should be as rapid as possible to a high degree of temperature, and that the wire should cool in a completely neutral medium or atmosphere.

The facts regarding annealing, as pointed out by the measurement of the magnetic capacity of iron wires, have no doubt been in a great measure perceived by ordinary mechanical methods. The results of the author's researches may be thus formulated:—

1. The highest degree of softness in any variety of iron or steel is that obtained by a rapid heating to the highest temperature less than fusion, followed by cooling in a medium incapable of changing its chemical composition.

2. The time required for gradual cooling varies directly as the amount of carbon in alloy.

Thus in absolutely pure iron rapid cooling, as in tempering, would not harden it, whilst steel might require several hours or days, even for pieces only one millimetre diameter. Slow cooling has no injurious effect upon iron, when cooled in a neutral field: consequently, where time is no object, we may employ slow cooling in every case.

A wire or piece of iron thoroughly annealed must not be bent, stretched, hammered, or filed; the hardening effect of a bend is most remarkable, and the mere cleaning of the surface by sand-paper hardens that surface by several degrees on the scale.

The following Table shows the effect of annealing upon a series of wires, kindly furnished expressly for these experiments by Messrs Frederick Smith & Co., of Halifax.

TABLE II.

Mark.	Description.	Magnetic Capacity.	
		Bright as sent.	Annealed.
		Degrees on Scale.	Degrees on Scale.
G	Best Swedish charcoal iron, 1st variety.....	230	525
F	Best Swedish charcoal iron, 2d variety.....	236	510
T	Best Swedish charcoal iron, 3d variety.....	275	503
S	Swedish Siemens-Martin iron.....	165	430
H	Puddled iron, best best.....	212	340
Y′	Bessemer steel, soft.....	150	291
Y	Bessemer steel, hard.....	115	172
Z	Crucible fine cast steel.....	50	84

From the above Table it will be seen that annealing had a great effect on the iron wires, doubling their value, and that Swedish iron stands far in advance of puddled iron; consequently, for the cores of electro-magnets in telegraph instruments—as in fact for all electro-magnets—Swedish iron is the most suitable, and the magnetic balance may find a field of practical utility in measuring each core before it is used in an electro-magnet, and may also aid us by its measurements in finding the best methods of annealing.

TEMPERING.

The influence of tempering upon the magnetic retentivity, or molecular rigidity, has been shown in every piece of iron or steel yet examined. Swedish iron hardens but 10 to 20 per cent. by tempering, whilst cast steel hardens 300 per cent.;* the molecular rigidity of tempered steel being 18 times greater than that of soft iron. The influence of different methods of tempering on crucible steel is shown in Table III., ranging from its ultimate molecular rigidity to its ultimate softness when annealed.

* For instance, in Table IV. below, the figure for Swedish iron No. 7 annealed is 525, tempered hard 415. On the other hand, the figure for cast steel annealed is 84, tempered hard 28. The reciprocals of these figures give what may be called a scale of hardness.

TABLE III.

Crucible Fine Cast Steel Tempered.	Mark.	Magnetic Capacity.
Bright yellow heat, cooled completely in cold water	A	28
Yellow red heat, cooled completely in cold water	B	32
Bright yellow, let down in cold water to straw color	C	33
Bright yellow, let down in cold water to blue	D	43
Bright yellow, cooled completely in oil	E	51
Bright yellow, let down in water to white	F	58
Red heat, cooled completely in water	G	66
Red heat, cooled completely in oil	H	72
Annealed	J	84

We may from this represent graphically a diagram which includes all methods of tempering; and another diagram which shall include all varieties of iron, from the softest iron to the hardest steel, intermediate qualities of hard iron and mild steel finding their place between the two extremes.

The numerous specimens of wires tested have been forwarded direct from the manufacturers, at the request of the author's friend, Mr. W.H. Preece, F. R. S., Electrician to the General Post Office. The chemical analyses of most of these wires have not been furnished; but Messrs. Frederick Smith & Co., of Halifax, not only supplied a beautiful series of wires, but had them specially analyzed by Mr. Henry S. Bell, of Sheffield, in order that the results should be as exact as it was in their power to make them. The author therefore neglects in this paper all other samples except those of Messrs. Frederick Smith & Co.: they all stand between, or are included by the two extremes, of Swedish iron and cast steel.*

Table IV. on next page gives the complete results of the mechanical, chemical, and physical tests upon these wires. The tensile strength and electric conductivity are as furnished by Messrs. Frederick Smith & Co., the chemical analyses are as given by Mr. Henry S. Bell, and the magnetic capacities of the bright hard-drawn wires, as of the annealed and tempered wires, were determined by the author with the aid of the magnetic balance.

* The author does not desire that Swedish iron should be considered as the softest of all possible irons, or tempered cast steel as the final limit of hardness. They are simply the limits found during these researches, but they may possibly be widened by a mere extended series of irons and steels.

Table IV. will aid us in drawing several conclusions. Taken in conjunction with Table III., it shows—

1st. That the degree of temper in cast steel is dependent jointly on the heat to which it is raised and the degree by which this is lowered in rapid cooling; the extremes in Table III. giving the relative molecular rigidity of the softest and hardest steel.

2nd. That a peculiar mild and homogeneous temper is obtained in oil.*

3rd. That the tempers or degrees of hardness, when steel is let down through the various colors, vary with the kind of steel tempered, as well as with the heat from which it has been let down..

In these experiments the author has noticed that the highest degree of temper has not been obtained with wires containing the relatively highest proportion of carbon. The maximum thus far was obtained with but 0.62 carbon; whilst in a series of steel wires, made expressly for these experiments, but in which the manufacturer stated only the amount of carbon, the results were as in Table V. on page 256.

It will be seen that the hardness as indicated in column "tempered" is not directly as the proportion of carbon; a marked example being the wire with 0.75 carbon, which is far softer than that with 0.62. The author might here have doubted the truth of the magnetic balance, if he had not previously verified its results by mechanical tests. In order hower to test the accuracy of the results, the wires S°

* The author has found, by a method more complicated than here described, and by the use of the induction balance, that all tempers heretofore tried (excepting these in oil) give a steel not homogeneous; and a temper let down to straw or blue has external strains differing from those of the interior.

TABLE IV.

Brand	Quality.	Electrical resistance per mile of .040 diam.	Tensile strength per square inch.	Magnetic Capacity.			Chemical Analysis.						
				Bright hard.	Anneal'd.	Tempered hard.	Carbon.	Silicon.	Sulphur.	Phosphorus.	Manganese.	Copper.	Iron.
		Ohms.	Tons.	Deg.	Deg.	Deg.							
G	Best Swedish charcoal iron, No. 1.	191.52	28	230	525	435	0.09	trace	trace	0.012	0.06	trace	99.69
F	Best Swedish charcoal iron, No. 2.	198.40	30	236	510	415	0.10	trace	0.023	0.045	0.03	trace	99.70
T	Best Swedish charcoal iron, No. 3.	199.62	31	275	503	395	0.15	0.018	0.019	0.058	0.234	trace	99.44
S	Swedish Siemens-Martin iron.....	226.32	34	165	430	390	0.10	trace	0.035	0.034	0.324	trace	99.60
H	Puddled iron, best best.....	259.92	30	212	340	328	0.10	0.09	0.03	0.218	0.234	0.015	99.11
Y′	Best homogene's soft Bessemer steel	266.52	35	150	291	255	0.15	0.018	0 092	0.077	0.72	trace	98.74
Y	Best " hard Bessemer steel	312.69	50	115	172	60	0.44	0.028	0.126	0.103	1.296	trace	98.20
Z	Fine crucible cast steel.....	350.08	55	50	84	28	0.62	0.06	0.074	0.051	1.584	trace	97.41

and Z were bound together, heated together to the same temperature, and plunged together in cold water; this was repeated several times, with the invariable result that the wire Z with 0.62 carbon was glass-hard and could not be marked by a file, whilst the wire S[s] with 0.75 carbon could be easily cut by the same file. Again we notice that in Table IV. the wires T, called Soft Swedish iron, contain precisely the same amount of carbon (0.15) as those Y′ in Table V. called Bessemer soft steel: but that whilst Y′ is comparatively hard when tempered, it does not become greatly softened by annealing. This is due probably to its greater proportion of some other ingredients. Similarly the wire S is much softer than H in Table IV., both having a similar amount (0.10) of carbon. The hardness of H when annealed is probably due to its greater proportion of phosphorus or some other substance.

It may be too soon to try and correlate the physical changes occurring in tempering with the corresponding chemical analyses: but the author believes that he has shown reason to hope that we may eventually obtain, by uniting chemical with physical analysis, a more clear insight into the mysteries of iron and steel.

DIVIDING LINE BETWEEN IRON AND STEEL.

Mechanical tests, as well as chemical analyses, have failed to find any distinct line of separation between the numerous varieties of iron and steel. The physical method which the author has employed shows clearly that there is no dividing line between iron and steel. If we glance at Table IV. we see that we have a continous series from the softest iron to the hardest steel, and between them we have every variety of intermediate quality. In point of fact the sixty brands which have been tested fill up all the gaps: and by their means we could choose irons gradually hardening into steel, or steel gradually softening into iron. Thus ordinary iron is physically a soft steel, and steel a hard iron. All are hardened by temper; all are hardened by mechanical treatment, as hammering and rolling; all are hardened by strains and stresses of any nature whatever: the difference, though large, is only in degree. At the extreme end towards iron, mechanical hardening has a greater effect than tempering. At

TABLE V.

	Mark.	Magnetic Capacity.		Carbon.
		Annealed.	Tempered.	
		Degrees.	Degrees.	Per cent.
Bessemer soft steel	Y′	291	255	0.15
Steel made for these Experiments	S′	348	206	0.40
" " " "	S^2	250	160	0.55
" " " "	S^3	209	133	0.60
" " " "	S^5	195	107	0.75
" " " "	S^4	144	61	0.65
Bessemer hard steel	Y	172	60	0.44
Fine crucible cast steel	Z	84	28	0.62

the steel end, tempering has a greater effect than mechanical hardening. We might here suppose we could find a physical dividing line: but the author has found some mild steels to stand just on that dividing line, which had previously appeared the most satisfactory. We are thus forced to adopt an arbitary line. Neither the mechanical nor physical methods will suffice to overcome the difficulty. Mechanically a certain tensile strength has been proposed—the objection to which is that unless we take note of the physical conditions (such as whether soft, tempered, &c.) we shall have very different magnetic readings for what would stand as the same material. The addition of the ultimate elongation might to some extent weaken this objection, but would not remove it. The physical method would allow us to fix upon a certain molecular rigidity, or difference in the readings of the same metal annealed and tempered, as the boundary; it would have, however, all the objection of being a purely arbitrary line. Chemical analysis also fails to show a dividing line, as the same proportion of carbon is accompanied by very different physical results, if sulphur, phosphorus, &c., are present. In the author's researches he has adopted the plan of simply reading an unknown piece of iron or steel in its annealed state: if the figure stands above 400° it is classed as iron, if below as mild or hard steel, according to its magnetic capacity. This happens to agree with the general classification at present in use, and suffices as a general division.

RELATIONS OF PHYSICAL FORCES IN IRON AND STEEL.

Iron is by far the richest of all metals in its physical nature. It stands almost alone in its magnetic qualities, as well as in its tempering properties, and, while there is an evident relation between capacity for temper and loss of magnetism when tempered,* so these experiments show an intimate if not absolute relation between the electrical conductivity of iron, and its magnetic capacity. In Table IV., in the column of electrical resistance as given by Messrs. Smith & Co., we find a progressive increase of resistance, just as we find a progressive decrease in magnetic capacity. And there is an exact correspondence between the two variations. The molecular rigidity, observed by the author as the cause of hardness, gives at once decreased magnetic capacity, and increased electrical resistance; so that from the magnetic capacity we might deduce its electrical resistance, and *vice versa*. A very remarkable phenomenon is that this only holds true in the limited sphere of elastic rotation, which the author has already described.

This demonstration the author believes to be of great theoretical value, and in a future paper, upon the theory of magnetism, its importance will be shown. In this paper the author has tried as far as possible not to bring theoretical considera-

*This is shown in Table IV. where the proportion of magnetism lost by tempering is seen to increase markedly as we pass from soft iron to hard steel.

tions forward; in the results presented we are dealing with proved facts.

Another extraordinary relation of physical to mechanical tests may be mentioned. In Table IV. the tensile strength bears no relation either to the magnetic or electric qualities. On increasing the electromotive force in the magnetic balance, all the readings became confused; there was no longer any fixed relation as to hardness, nor any other quality. But on again forcing the magnetism to a very high point, the figures for magnetic capacity were found to bear exactly the same relation to each other as those for tensile strength. This, however, may have been only an accident, as it only seems true at present in relation to the wires in Table IV.; but it gives hope that by a new method we may some day be enabled, not only to deduce electrical conductivity from magnetic capacity, but also tensile strength. Already in Table IV. we notice a close relation between molecular rigidity, as indicated by the figures for the annealed wires, and tensile strength.

Leaving aside all theoretical considerations and hoped—for improvements in the methods of observation, the author believes that he has demonstrated clearly that, by the aid of the instrument and methods described, we can at once determine the physical state of iron, as influenced by tempering and mechanical hardening, from the ultimate degree of softness to that of hardness; that we can at once determine the best iron for electro-magnets, and the best methods of softening it, as well as the best steel for permanent magnets, and the best temper to be given to it. He therefore ventures to hope that the Magnetic Balance will prove an aid of no small value in all researches into the physical state of iron and steel.

THE UNIFICATION OF LONGITUDES AND OF TIME—RESOLUTIONS OF THE INTERNATIONAL GEODETIC COMMISSION.

From "Science."

The seventh general conference of the International Geodetic Association held at Rome, and at which representatives of Great Britain, together with the directors of the principal astronomical and nautical almanacs and a delegate from the Coast and Geodetic Survey of the United States, have taken part, after having deliberated upon the unification of longitude by the adoption of a single initial meridian, and upon the unification of time by the adoption of a universal time, has agreed upon the following resolutions:—

1°. The unification of longitude and of time is desirable as much in the interest of the sciences as in that of navigation, of commerce, and of international communications. The scientific and practical utility of this reform far outweighs the sacrifice of labor and the difficulties of rearrangement which it would entail. It should, then, be recommended to the governments of all the interested states to be organized and confirmed by an international convention, to the end that hereafter one and the same system of longitudes should be employed in all institutes and geodetic bureaus, for general geographic and hydrographic charts, as well as in astronomical and nautical almanacs, with the exception of those made to preserve a local meridian; as, for instance, the almanacs for transits, or those which are needed to indicate the local time, such as the establishment of the port, etc.

2°. Notwithstanding the great advantages which the general introduction of the decimal division of a quarter of the circle in the expressions of the geographical and geodetic co-ordinates and in the corresponding time-expressions is destined to realize for the sciences and their applications, it is proper, through considerations eminently practical, to pass it by in considering the great measure of unification proposed in the first resolution.

However, with a view to give satisfaction at the same time to very serious scientific considerations, the conference

recommends, on this occasion, the extension, by the multiplication and perfection of the necessary tables, of the application of the decimal division of the quadrant; at least, for the great operations of numerical calculations for which it presents incontestable advantages, even if it is wished to preserve the old sexagesimal division for the observations, for charts, navigation, etc.

3°. The conference proposes to governments to select for the initial meridian that of Greenwich, defined by a point midway between the two pillars of the meridian instrument of the observatory of Greenwich; for the reason that that meridian fulfills, as a point of departure for longitudes, all the conditions wished for by science, and because, being at present the best known of all, it offers the most chances of being generally accepted.

4°. It is suitable to count the longitudes, starting from the meridian of Greenwich, in the sole direction from west to east.

5°. The conference recognizes for certain scientific wants, and for the internal service in the great administrations of routes of communication—such as the railways, steamship-lines, telegraphic and post routes—the utility of adopting a universal time, along with local or national time, which will continue necessarily to be employed in civil life.

6°. The conference recommends as the point of departure of universal time and of cosmopolitan dates, the mean noon of Greenwich, which coincides with the instant of midnight or with the commencement of the civil day, under the meridian situated twelve hours, or a hundred and eighty degrees, from Greenwich.

It is agreed to count the universal time from 0 hour to 24 hours.

7°. It is desirable that the States which, with a view to adhere to the unification of longitudes and of time, find it necessary to change their meridians, should introduce the new system of longitudes and of hours as soon as possible.

It is equally advisable that the new system should be introduced without delay in teaching.

8°. The conference hopes that, if the entire world agrees upon the unification of longitudes and of hours by accepting the meridian of Greenwich as the point of departure, Great Britain would find in this fact an additional motive to make on its part, a new step in favor of the unification of weights and measures by adhering to the *Convention du metre* of the 20th of May, 1875.

9°. These resolutions will be brought to the knowledge of the governments, and recommended to their favorable consideration, with an expression of a hope that an international convention—such as the government of the United States has proposed—for confirming the unification of longitudes and of time should be decided upon as soon as possible.

REPORTS OF ENGINEERING SOCIETIES.

AMERICAN SOCIETY OF CIVIL ENGINEERS.—The last issue of the transactions contains:

No. 266. The cost of steam power, by Chas. E. Emery.

No. 267. The Shubenacadis Canal, Edward H. Keating.

No. 268. The Nasmyth Pile-driver, Don J. Whittemore.

No. 269. The effect of passing trains on iron bridges, masonery, etc., James L. Randolph.

No. 270. An economical and efficient railroad bridge floor, W. Howard White.

ENGINEERS' CLUB OF PHILADELPHIA.—Regular meeting January 19th, 1884.

Col. William Ludlow, President, in the chair.

After calling the meeting to order, President Ludlow said:

"I deeply regret, gentlemen, that almost my first official duty as your President is to appear as a harbinger of ill tidings, and to announce to you the death of our past President, Mr. Strickland Kneass; a man whom you all knew, and knowing, held in the highest esteem—a typical good citizen and good engineer, faithful in every relation of life, full of industry and conscientiousness, devoted to duty, warm-hearted, clear-headed, capable and honorable.

A special meeting of the Board was held on Thursday, for the consideration of the best method by which our appreciation of Mr. Kneass, and our deep regret at his loss, might be expressed for transmission to his friends and incorporation into the records of the Club.

It was thought fitting to substitute for the drafting of the ordinary resolutions, which bear, perhaps unavoidably, a somewhat perfunctory character, the preparation of a Memorial, containing such a recital of the history of Mr. Kneass as should more effectively set forth his distinguished services and example, both to the profession and to the community.

The Special Committee to whom this duty is assigned, consists of Messrs. Graff, Chairman, DuBarry, Worrall, McClure and Dye, the gentlemen best qualified to discharge it."

Mr. Wilfred Lewis read a paper upon the "Resilience of Steel," reviewing some of the

means employed for the storage of energy, and showing the place occupied by steel among them.

Among the means now employed, compressed air, hot water and the storage battery were cited from an English writer as being about equal in value, and as giving out about 6,500 ft. lbs. of work per pound of material used.

Steel springs, according to the same writer, were said to yield about 18 ft. lbs. per pound. In this connection, the project of using steel springs as a motor for street cars was referred to as the most hopeless of all possible means of locomotion.

To test the accuracy of this statement in regard to steel, several experiments were made by the writer upon tempered specimens, both for tension and flexure. Contrary to expectation, the highest results were shown by the flexure of a small spiral clock spring weighing 2040 grains, which gave out, when wound up, about 45 ft. lbs. of energy, or in other words, 154 ft. lbs. per pound.

The transverse strength of this steel within the elastic limit was found to be about 300,000 lbs. per sq. in., and its modulus of elasticity about 30,000,000. Such extraordinary strength, with such a low modulus, was so far beyond conjecture that it seemed to give a new hope for the success of the project referred to; but after making the necessary allowances for weight of car and efficiency of driving mechanism, it was found that not more than about 20 ft. lbs. per pound of car would be available for locomotion. It was therefore improbable that such a car could ascend a hill over 20 feet high.

It was also a matter of doubt whether larger springs could be made to show results which would even approach these figures, and on this account the experiments about to be tried might be looked for with some interest.

Mr. H. C. Luders presented a description, illustrated by photographs, of the ancient ship found near Sandefjord, in Norway.

He also exhibited specimen of rolled and annealed phosphor-bronze of maximum ductility, and consequently of minimum tensile strength, and submitted the following data of the test thereof: length, 2″: diameter, 0.57″; subjected to a strain of 13,620 lbs., equivalent to 53,400 lbs. per sq. in.; elongation, 70.5 per cent.; reduced area at point where fracture would occur, 0.3″; elastic limit, about 18,000 lbs. per sq. in. Hard rolled rods tested without turning off the surface, have shown a tenacity exceeding 90,000 lbs. per sq. inch.

The Secretary presented, for Mr. Louis C. Madeira, Jr., the Record of American and Foreign Shipping, containing an interesting set of drawings for the details of construction of iron ships.

Mr. Percival Roberts. Jr., gave some account of the results of experiments, now being conducted by Mr. James Christie, at Pencoyd, upon the relative elasticity of iron and steel structural shapes.

REGULAR MEETING, FEBRUARY 2, '84.

President Wm. Ludlow in the chair.

Mr. Wm. Lorenz presented a comprehensive discussion of steel and iron railroad ties, illustrated by numerous full-sized models and drawings.

Mr. C. J. Quetil, introduced by Mr. J. J. de Kinder, gave an illustrated description of his wire truss railway.

President Wm. Ludlow exhibited specimens of obstructed water pipe, discovered in his practice, and explained the chemical causes which led thereto.

Mr. J. Milton Titlow exhibited and described a full set of general and detail drawings for the proposed stone bridge over the Schuylkill River at Market Street, Philadelphia. As the cost thereof is estimated at $1,200,000, in the discussion which followed, President Ludlow suggested the alternative of building two $600,000 iron bridges therewith—one at Market Street and one at Walnut Street, over the Schuylkill, the latter bridge being very greatly needed at this time.

Mr. E. F. Loiseau presented a description of the process of manufacture of Portland cement by the application of his solidifying apparatus.

Mr. Robt. W. Lesley, visitor, supplemented Mr. Loiseau by explaining the difference between the old and the above-mentioned methods in actual practice.

ENGINEERING NOTES.

THE BROWN HOISTING APPARATUS.—Briefly described, the Brown bridge tramway is as follows: It is a trestle bridge supported on movable piers. In the rear of the docks the piers are placed upon double tracks and so arranged as to be transferred easily along the docks. One great improvement in the application of this latest patent is the ability to move either the rear or the front piers separately or with each other. The tramway over the front pier is arranged with a "U" brace, on a ball and socket movement, thus allowing any position of the bridge for the convenience of loading or unloading without causing a strain upon any of its parts. The front pier is run upon a single track for this purpose. This pier is thirty feet high to the bridge, which slopes gradually upward to a height of forty-five feet over the rear piers, which are erected through an engine-house 40x16 feet in dimensions at their base. In the plant now erected are three bridges, two of which are connected with the engine-house, as it is usually unnecessary to move them more than a few feet in front to accommodate themselves to the rear hatches of a vessel. A third bridge is entirely detached and is used in emptying the fore hatch. The total length of the stringers is 210 feet, and the bridge has a clear span of 180 feet. Its bottom cords are of wrought iron; the top cords and stringers of wood, amply braced for its own weight and the wind pressure, besides the weight of the load, and in such a manner as to permit of a free movement of the bucket. The apron extending over the ship is raised to escape masts and rigging when not in operation, The distinctive feature of the apparatus and one not possessed by any other hoisting and conveying machines is the ability of the operator to hoist and lower at the upgrade end. Its operation s

are performed in a positive and automatic manner without aid from springs by the direct action of the hoist rope, and is thus placed under the perfect control of the operator. The capacity of the machine is limited only by the speed with which the men on ship board can load the buckets, but with the usual speed now attained, and, with three buckets for each hatch, it is an easy matter to get out 1,000 tons of ore in ten hours.

THE Cleveland Institution of Engineers held the third meeting of the present session at Middlesbrough on Monday last. Mr. G. J. Clarkson's paper on the new patent law, read at the previous meeting, was thoroughly discussed, and subsequently Mr. Lowe, of Haswell Colliery, read an interesting paper on a new "mechanical coal-getter." The object of this apparatus is to supersede the use of gunpowder in breaking down the coal after it has been undercut. Holes must be made at the top of the seam as usual. Then the instruments—which are long and narrow—are introduced, one into each hole. By turning a crank attached to the outer end, the collier has the power of thrusting apart two loose blocks near the inner end of the apparatus, by forcing a wedge between them. The result is to separate the whole block of coal under treatment from the strata immediately overlying it. By means of this invention far less small is made than where gunpowder is employed; and besides this advantage, there are the still greater ones of avoiding any vitiation of the air, and of running no risk of firing any combustible gases which may have accumulated in the vicinity.

THE subject of fuel economizers was before the members of the Manchester Association of Employers and Foremen, at their usual fortnightly meeting on Saturday. The question was introduced in a paper read by Mr. Daltry, of Manchester, who urged that nothing like the best possible results had yet been attained by the economizers at present in use. If they supposed that with any economizer the temperature at which the issuing gas entered the chimney was as low as 400 deg., there was still a deal more of heat wasted than if it were possible to let them emerge at the temperature of the feed-water. By a rough calculation he estimated that this would give an increase of economy of 15 per cent. of the coal burnt; and supposing that with a forced draught and a suitable economizer this result could be attained, nothing could better it. A somewhat animated discussion followed. Mr. Nasmith expressed the opinion that the present form of economizer was not a perfect one, nor did he think that any form of economizer would be perfect until they got a more efficient system of heating their boilers. They would have to adopt some method other than the use of green fuel before they could apply any system of economizing the waste heat with the best results, and in his opinion they would only be able to effect this by the use of gaseous fuel. There was a wide field not only for a radical alteration in the fuel economizer itself, but also in the method of raising and applying the heat. Mr. Marmock did not see that much advantage was to be gained by the use of gaseous fuel: in some cases the use of gaseous fuel had given worse results than direct firing with coal. Mr. Baldwin was of opinion that under certain conditions gas fuel might be used with great benefit in the heating of boilers. The chairman—Mr. Thomas Asbury, C.E.—thought there were many points in connection with fuel economizers that would have to be considered all round; the whole subject of fuel economy was one about which they had been rather careless, and in his opinion engineers had been rather remiss in taking advantage of every possible point. With regard to gas fuel, he thought the time was fast hastening when this method of raising heat would become pretty general.

THE FORTH BRIDGE.—Considerable progress is now being made with the works of the great bridge across the Firth of Forth, which includes two spans of the unprecedented width of one-third of a mile each, or about four times that of any existing railway bridge. Some £30,000 per month is the present expenditure in temporary and permanent works, at which rate the bridge will be completed in the anticipated period of five years from the date of the contract. Only four classes of materials will be used in the permanent works of the bridge, and these being steel, granite, whinstone, and Portland cement, they will be of the most durable and trustworthy character. Some idea may be formed of the magnitude of the undertaking from the statement that the materials required for the Forth Bridge would fill 1,000 goods trains of average length and capacity. This enormous weight will, of course, require to be handled several times, the whinstone alone being found on the spot, the granite being shipped from Aberdeen, the steel from Glasgow and South Wales, and the Portland cement from different places in England. As it is intended to manufacture the steel superstructure of the bridge on the spot, very extensive works have been constructed at Queensferry, and the plant provided includes about fifty steam engines of various classes, and a large number of specially designed hydraulic appliances, drilling machines, and other tools for dealing with the 45,000 tons of steel which will be used in the bridge.

At the present time ten of the smaller piers have been founded and carried up to varying heights, and the rock has been prepared by the aid of diamond drills and rock drills of other types for the founding of some of the main piers. Each of the three main piers will consist of a group of four cylindrical masses of granite and whinstone, ranging from 70 feet to 60 feet in diameter at the base, and founded either upon rock or upon boulder clay of an exceptionally hard character. Where the bed of the river is of rock, it will be leveled and otherwise prepared for the piers by means of a large diving bell of special construction, wherein are a series of rock drills driven by compressed air, the whole being lighted by incandescent electric lights. Where the foundation is on clay, a considerable thickness of mud and silt has to be cut through, and preparations are now complete for putting in the first of the

piers by what is known as the pneumatic process. A caisson, or diving bell, 70 feet in diameter, and 65 feet in height, will be sunk to the required depth by a large number of men working in the bell, which will, of course, be constantly fed with compressed air, to prevent the water from flowing into the chamber, and to supply the men with the required amount of oxygen. Many bridge piers and other subaqueous works have been successfully carried out on this plan, but in the present case the appliances for removing the earth and the details generally are of a novel and interesting character.

Simultaneously with the building of the piers, the manufacture of the superstructure of the bridge will be proceeded with, and several hundreds of tons of steel plates are now on the ground. One of the peculiar features of the Forth Bridge is that all the important members subject to compression will be of a tubular form—a form which the instinct of an ordinary person and the deductions of theory and experiment alike indicate as best adapted to resist compressive stress, but which difficulties of manufacture have excluded from nearly all but American bridges heretofore. About three miles of steel tubes, ranging from 12 feet to 5 feet in diameter, and from 1¼ inch to ½ inch in thickness, will be required for the Forth Bridge, and this great quantity justifies the expenditure which has been incurred in the special plant for the manufacture of the tubes. The proper plant being provided, it is as simple to make a tube as any other shaped member. Thus the steel plates will be heated in gas furnaces, and, when red hot, be stamped to the desired curvature in a 2,000 ton hydraulic press; the edges will next be planed all round and the plates be temporarily clamped together to form a tube about 400 feet in length. Traveling drilling machines will then traverse the tube and drill all the holes required to rivet the plates together, but this riveting will not be done until the bridge is being erected, plate by plate, across the Forth. All of the machinery required to commence the manufacture of the tubes is on the ground, and has been found to work very satisfactorily, which reflects credit on the designer, Mr. Arrol, one of the contractors of the bridge.

To fix the position of the piers of a bridge having two clear spans of 1,700 feet, and two of 700 feet, across a channel 200 feet in depth, is not so simple a problem as may appear to some persons. In the case of the Forth, the banks are more or less precipitous, and the country is undulating, so that it was difficult to get a base line for a trigonometrical measurement. At the request of Mr. Fowler, the Ordnance Department kindly sent a party of sappers to recover the old stations used in the Ordnance Survey of Scotland, and to take the necessary additional observations to determine the width across the Firth of Forth. The calculated result differed about 1 foot from that obtained by Messrs. Fowler and Baker's engineers, which is not much, perhaps, in a difficult measurement of over a mile across deep water, but enough to make it desirable that it should be set right. Mr. Baker held the view that it was possible to measure the 1,700 feet span to the eighth of an inch if necessary, and the measurement has just been made without any difficulty whatever. A length of 1,700 feet was first measured with what may be termed absolute exactitude along a straight piece of the North British Railway, and posts of a certain height were firmly fixed at each end. A steel pianoforte wire was then hung from these posts with a sag or droop of 24 feet at the center, and marks were fixed to each end of the wire to indicate the 1,700 feet length. The wire was then coiled on a roller and taken on board a steam launch, from which it was laid like a submarine telegraph cable across the channel of the Forth. It was then hauled up above the surface of the water till it hung with the original droop of 24 feet, when, assuming the temperature to be the same, the marks put on the pianoforte wire should indicate the required span of 1,700 feet. This process was repeated several times, and the wire taken ashore to the railway to test whether it had stretched, but all the observations were in exact accord. The sensitiveness of this simple mode of measuring the span with a wire weighing only 8 lb. in all was quite astonishing. Thus, when Mr. Baker slacked out the wire only ¼-in. in the 1,700 feet, Sir Thomas Tancred, one of the contractors, who was in a boat in the center of the Forth, at once signaled there was too much droop in the wire. At another time he signaled that it was too high, but a reference to the thermometer showed that the temperature had fallen 2 deg., so that the wire proved itself a most delicate indicator of changes of temperature. The officers and crew of Her Majesty's ship *Lord Warden*, the ironclad guardship anchored immediately above the site of the bridge, have always taken much interest in the works, and while the measurement was being made, they very kindly manned their boats and regulated the traffic past the spot. Engineers and contractors engaged in carrying out a giant bridge across a stormy estuary and reaching to a height twice as great as that of the topgallant masts of the *Lord Warden*, could desire no better neighbors than Captain Kennedy and his well-disciplined and willing crew of 300 bluejackets.—*Times.*

IRON AND STEEL NOTES.

BLOWHOLES IN STEEL.—The winter session of the Manchester Association of Employers, Foremen and Draughtsmen was opened on October 13, when a well-attended meeting was held in the Mechanics' Institution. An interesting paper on "The Manufacture of Steel" was read by Mr. W. Annable, of Govan, Glasgow. The paper was mainly devoted to a discussion of the best methods to be adopted in overcoming one of the serious difficulties—the existence of blowholes—in the manufacture of steel. Mr. Annable, having quoted several authorities to prove that gases were occluded as well as mechanically mixed with the metal, said they were told that the mechanically mixed gases tried to escape during the time the metal was passing from the fluid to the solid state, but as steel set so rapidly, a goodly portion of this

gas was entrapped and formed blowholes. This theory appeared very feasible, as cavities were more numerous near the top end of ingots than at any other part, and this seemed to demonstrate that, if the metal had remained fluid a little longer, these gases would have escaped. In practice, however, this was not the case, for metal that was not "dead melted" and contained these gases was always the longest in setting, and instead of going down in the mould as contraction took place, did the reverse, began to come up, and, unless force were applied, would come over the top of the mould. As it seemed to be the mechanically mixed gases that gave us the blowholes, these were the gases that makers and users of steel were interested in, and to overcome the effects of which many methods had been adopted. Having described several of the best known of these methods, Mr. Annable said his own experience told him that, if the steel were well saturated with any of the five following properties– carbon, manganese, silicon, phosphorus and sulphur—which were always associated with iron and steel more or less, the metal would sink down in the mould, and would be found quite free from blowholes. He believed that iron founders found the same thing, that common iron was easier to deal with in casting than the better brands were. But this metal, although free from "blows," would not be suitable for our everyday requirements, as we must have a definite composition for each and every purpose. Consequently the important question arose as to how they were to obtain these definite tempers without blowholes. What was needed was a simple, practical method whereby they could produce all classes of steel free from blowholes or cavities. He had tried several methods with success. One was dead melting, but this did not always give the desired results when dealing with large masses of metal, as they could not get rid of the metal before the temperature became reduced, and this gave rise to some changes. With small quantities, however, as pot metal, dead melting was all that was required; it answered admirably, and could be depended upon. One method he had tried, and which he would recommend, was to cast the ingots in closed top moulds. These were run from the bottom, and inside each mould a small quantity of combustible material was placed, which was fired by the incoming metal. This at once expanded the air in the mould and created a pressure, as it could not readily escape by the holes in the top of the mould, which were only two, each $\frac{3}{8}$-inch, whilst the head of metal they had in the runner counterbalanced the pressure, and the gases rushed out of the two small openings with a loud roar. It was not, however, pressure to which he wished to draw their attention, but to one fact, that the metal was being cast free from atmospheric air, to which Dr. Muller and others assigned the cause of blowholes. The metal was being cast in an atmosphere of carbonic acid, brought about by the combustion of the shavings which were placed in the moulds before casting began. Besides, they had all the ingot cast at one temperature, one density, and it would be found perfectly homogeneous, which could not be said of those cast from the top, whilst it would be also free from blowholes. In addition, the "pipe" in the top of the ingot would be found much reduced, according to the length of the runner, as the ingots received a supply of metal from it during the time they passed from the fluid to the solid state. Another method was to place the ladle on to the center runner, and then they excluded all air besides using the weight of metal in the ladle to force the metal home against the pressure of gas set up in the mould. The open-topped mould, so much used for Bessemer and Siemens ingots, was objectionable in more than one sense. When the metal fell from the ladle to the bottom of the mould, sometimes 8 or 9 feet, it not only took down with it a stream of air, but in falling on the bottom of the mould, the first twenty or thirty pounds of metal were splashed in all directions, and if they were to examine these splashes they would find them to be covered with a blue scale, which was oxide of iron. As they went on pouring the hot fluid metal, it re-melted the splashes, and quite a reaction took place in the steel at the lower end of the ingot; the oxide of iron gave up its oxygen, and attacked the carbon and manganese of the steel, just the same as when ore was thrown into the furnace, and they could at any time see the brownish red fumes leave an open-topped ingot mould when casting. These fumes, which were the result of the chemical action below, became less and less as the metal got nearer the top. He had stated that open-topped ingots were not homogeneous. To prove that this theory was correct, he had several ingots analyzed, and the result showed that at any rate carbon was eliminated by the reaction of which he had spoken, caused by the interference of the atmospheric air. Some might think he had exaggerated the effects of oxygen on molten or heated metals, but what he had said would be confirmed by the practical experience of any smith, as a small bar of iron or steel at a red-heat under the hammer would yield a thin film of scale at every blow. This scale was oxide of iron, and showed with what avidity oxygen attacked metals even at a red-heat. They might, therefore, assume that, with fluid metal broken up or disintegrated, as it was when falling some feet to the bottom of the mould, oxidization would take place at a greater ratio than when metal was only just red hot. He had seen ingots cast in the manner just described. When brought under the hammer, the bottom end had dropped off at the first blow, after which the remainder of the ingot had hammered all right and given good blooms and billets. This he attributed to the manner in which the ingot was cast. What they wanted was some simple method of treatment, which would give them all tempers of steel free from cavities. Pot metal, which gave them all kinds of tools, they could get free. Castings also might be made free by conforming to certain treatment and chemical mixture, by skill and care in preparing the moulds, and by judgment in allowing for fluid contraction by giving the metal plenty of head large enough in diameter. As the bulk contracted in cooling, it would get its feed or supply from the center of this head or column of metal put there for the purpose, and by mak-

ing provision in coils for that large amount of contraction in steel, this would take place without tearing the casting asunder. The ordinary Bessemer metal was very frothy and lively when cast below carbon 0.3, mild steel temper, and it was with difficulty that it was kept in the mould; but even should it be prevented coming over the top, it was not free from blowholes, which would be found to penetrate to half the depth of the ingot. Solid castings were being produced in steel, but the great difficulty arose where they had to make ordinary soft steel free from blowholes, and this was a difficulty which led to enormous waste in manufacturing operations. The subject was, however, receiving the fullest attention, and he had not the least doubt but that the difficulty would be shortly overcome, and that they would be enabled to produce ingots and castings that would be as sound and free from cavities as though they had been forged under the hammer.

A discussion followed the reading of the paper, and Mr. Annable, replying to a few questions, said a good reliable cylinder could not be made without putting a large head upon it; if it were made in any other way, it was sure to be a failure on account of the contraction which took place all the way down. In his opinion the steel made by compression was really no stronger than steel made in the ordinary way, provided, of course, that it received proper treatment afterwards. He believed the report with reference to the large output of steel in America was perfectly true, but, speaking for himself, he would only say that he had never gone in for quantity, but for quality. He did think they could get a reliable tool steel from the Bessemer process, because the proportion of manganese was too great; and they could not get the amount of carbon which was required. Tool steel must have its chemical elements mixed to a nicety, because if this were not secured, it would be unsuitable for the purpose. The chairman, in closing the discussion, observed that the difficulties connected with the manufacture of steel were being grappled with in a manner which gave them confidence in ultimate success. Good sound ingots and sound castings were the great desideratum in the use of steel, and he had no doubt the time would soon come when solid steel would be relied upon quite as much as in any other metal. The varieties of steel were so great that it was sometimes difficult to discover where steel began and where iron ended. One great mistake that was made was that the user of steel did not always specify the purpose for which the steel was required, and if engineers or other persons designing anything where steel was required would be frank and tell the maker what the steel was required to do when it was in position, they would frequently get more satisfactory results. With the tools of such immense power as were now being produced, many of the difficulties which had hitherto stood in the way of the manipulation of steel would disappear.—*Iron.*

RAILWAY NOTES.

THE railways of South Australia bring in a total revenue of £469,000, yet the actual net receipts to be set against the interest on cost of construction is only £146,000. The *Colonies and India* says economical management might make these railways indeed a splendid property, equal in value, perhaps, to three-quarters of the national debt. But as things are at present it can only be said that they pay about half the interest on their cost of construction.

ORDNANCE AND NAVAL.

THE many erroneous statements made in the daily and weekly papers with regard to the efficiency of primary batteries, induces me to send the following figures, relating to the quantity of fuel required to produce 1 horse-power per hour by means of steam, and by electricity. Perhaps these figures may be thought of sufficient interest for insertion in your next issue.

		Foot-pounds.
1 Heat-unit (Fahrenheit).....	=	772
Ditto (Centigrade).....	=	1,390
1 Horse-power per minute.....	=	33,000
Ditto per hour.....	=	1,980,000

Ditto ditto in heat-units—

$$\frac{1,980,000}{1,390} = 1,425$$

Heat-units (Centigrade) contained in—

1 lb. of coal.....	=	8,000
1 lb. of hydrocarbon (liquid).....	=	11,000 (about)
1 lb. of hydrogen.....	=	34,000

Assuming with Professor Adams (see opening address at the Society of Electricians), the greatest efficiency of a theoretically perfect heat-engine to be = 23 per cent. We have—

for coal.....	23 per cent. of	8,000	= 1,840
for hydrocarbon	" "	11,000	= 2,530
for hydrogen....	" "	34,000	= 7,820

Hence, 1 horse-power requires for its development in such a theoretically perfect engine at least—

$$\frac{1,425}{1,840} = 0.774 \text{ lb. of carbon,}$$

$$\text{or } \frac{1,425}{2,530} = 0.563 \text{ lb. of hydrocarbon,}$$

$$\text{or } \frac{1,425}{7,820} = 0.182 \text{ lb. of hydrogen.}$$

The efficiency of a perfect voltaic battery and electric motor may, at the lowest estimate, be assumed as 90 per cent.

1 lb. of zinc = 1,300 J; 90 per cent. = 1,170. One horse-power developed in an electric motor by a current from a voltaic battery requires hence at least—

$$\frac{1,425}{1,170} = 1.133 \text{ lbs. of zinc.}$$

The above will elucidate the assertion about 1 horse-power being produced by 0.166 lbs. of hydrocarbon, against the lowest possible 0.563 lbs.; but it also shows Mr. Reckenzaun's modest estimate of 2 lbs. of zinc per horse-power per hour in its proper light.

I may here add that D'Arsonval (see *La Lumiere Electrique*, vol. v., p. 90) has obtained 368 kilogramme metres with 1 gramme of zinc: this corresponds to a consumption of 1.42 lbs. of zinc per horse-power per hour.

C. GODFREY GUMPEL.

—*In Journal of Society of Arts.*

BOOK NOTICES

THE AIR WE BREATHE, AND VENTILATION. By HENRY A. MOTT, JR., Ph. D., F.C.S. New York: John Wiley & Sons.

It requires but little technical knowledge to read this book understandingly. It is a small volume, but has a good deal of practical science condensed within its leaves.

In the portion relating to Ventilation, a large proportion of the space is devoted to illustrations of methods and apparatus employed in aiding or forcing ventilation.

NOTES ON ELECTRICITY AND MAGNETISM. By J. B. MURDOCK. New York: MACMILLAN & Co.

This is the book of an instructor who finds that students require additional aids to those furnished by such text books as Sylvanus Thompson's Elementary Lessons.

Direct reference is made to Prof. Thompson's book, and the subjects are treated in the same order.

It will doubtless prove a valuable supplement to the larger book.

ENERGY IN NATURE. By WM. LANT CARPENTER, B.A., B. Sc. London: Cassell & Company.

This is a popular treatise on Physics, and is especially designed by its style to interest the general reader.

The separate chapters treat successively of —Force and Energy, Heat, Combustion, Electricity and Chemical Action, Magnetism and Electricity, Energy in Organic Nature.

TABLES FOR CALCULATING THE CUBIC CONTENTS OF EXCAVATIONS AND EMBANKMENTS. By JOHN R. HUDSON, C.E. New York: John Wiley & Sons.

The author's method of calculation is new and improved. The tables are well printed, and constructed for road-bed-widths of 10, 14, 18, 24 and 28 feet, and for slopes of 1 to 1, 1 to 1½ and 1 to 1¼.

The book is of more convenient size than most similar treatises.

THE WINE-PRESS AND THE CELLAR. By E. H. RIXFORD. New York: D. Van Nostrand.

This is designed as a practical guide to the vine culturist who designs also to make wine. Although the treatise is small, no necessary detail seems to be omitted.

The subjects treated in separate chapters are: Gathering Grapes, Must, Sugaring and Watering the Must, Stemming and Crushing, Fermentation, Red Wine, White Wine, Casks, Sulphuring and Aging, Cellars, Racking, Clarification, Sweet Wines, Defects and Diseases, Wine in Bottles, Cutting or Mixing Wines, Wine Lees and Piquette, the Composition of Wine, Miscellaneous.

The advice is adapted to American Viticulturists, especially those of California.

KNIGHT'S NEW AMERICAN MECHANICAL DICTIONARY. Section Four. Boston: Houghton, Mifflin & Co.

The present section includes the articles from Printing-Press to Zoogyroscope, and completes the edition. The five years that have elapsed since the former edition was completed have produced an abundance of material with which to enlarge the work. Indeed, the superabundance of material led to the adoption of an entirely new feature in the present work—that of references to technical journals which contained fuller accounts of late inventions.

Fifty-six full page illustrations and numberless smaller ones now illustrate the text.

It is an indispensable aid to the mechanical engineer.

A NEW SYSTEM OF LAYING OUT RAILWAY TURNOUTS. By JACOB M. CLARK. New York: D. Van Nostrand.

This useful little essay is accompanied by tables which will enable the practical engineer to locate a turnout instantly. The text gives the mathematical demonstrations upon which the tables are based. The author's method is intended, so far as it differs from others, to save time and labor.

The solutions heretofore published regard the turnout track as located on a curve which is tangent to a switched or deflected rail. It is generally more convenient to locate the turnout upon a curve which is tangent to the main track at a point not far from the heel of the switch. The head-block is then placed where the departure of the center lines from each other is equal to the necessary deflection or throw of the switch bar, which, in turnouts from straight tracks, should not be less than half, nor more than the entire distance from the head-block back to the tangent point.

In this way the exact solutions for all but very rare cases are reduced to three, each of which involves a simple case of plane trigonometry.

MISCELLANEOUS.

M. DUCHARTRE was led, by the influence which a light of very feeble intensity exercises upon heliotropic movements, to vary some experiments by using moonlight. He sowed seeds of plants which were very sensitive to light, such as *Lens esculenta*, *Ervum lens*, *Vicia sativa*. When the plants were a few centimetres in length he put them in a dark place, where he kept them until the night of the experiment. The stalks became slender, long and white; the leaves developed slightly with a light yellowish tinge. On three successive nights when the sky was exceptionally clear the plants were placed behind a large window with a southern exposure, so that they received the direct light of the moon from 9 p. m. to 3 a. m. According to the *Comptes Rendus*, from the very beginning of the exposure the stalks began to bend, so as constantly to present their concavity and the terminal leaf bud to the moon, following it in its course.